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AN

EXPERIMENTAL INQUIRY

INTO

THE LAWS

OF THE

VITAL FUNCTIONS,

WITH

SOME OBSERVATIONS ON THE NATURE AND TREATMENT

OF

Internal Diseases;

BY

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IN PART RE-PUBLISHED,

BY PERMISSION of the PRESIDENT of the ROYAL SOCIETY,

FROM THE

PHILOSOPHICAL TRANSACTIONS OF 1815 & 1817,

WITH

THE REPORT OF THE INSTITUTE OF FRANCE ON THE EXPERIMENTS OF M. LE GALLOIS.

SECOND EDITION,

WITH SOME ADDITIONAL OBSERVATIONS.

LONDON:

PRINTED FOR THOMAS AND GEORGE UNDERWOOD, FLEET-STREET;

AND ADAM BLACK, EDINBURGH.

1818.

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Philip

London: Printed by W. CLOWES, Northumberland-court, Strand.

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PREFACE

TO THE

SECOND EDITION.

IT must be gratifying to an author under any circumstances, that a second edition of his work should be called for in less than nine months. To me, who have been accused of displaying in this Treatise a want of the common feelings of humanity, it is peculiarly so. It is an additional evidence of the truth of a maxim on which I have ever firmly relied, that a man whose only aim is to act on the principles of correct feeling and common sense, however he may be assailed by the prejudices of some individuals and the injustice of others, will always have the voice of the majority. That such has been my aim, I shall not, I hope, find it difficult to prove.

So much has lately been said by some, who believe that they advocate the cause of humanity, and particularly by some Members of the Royal Society, against the employment of living animals in physiological experiments, that I wish, as far as I am able, to call public attention to this question; convinced, that wherever it becomes the subject of calm and deliberate reflection, all difference of opinion respecting it will cease. I shall, therefore, in an Appendix to this Treatise, enter on it at some length. I shall there also reply to insinuations of a different kind, though connected with the same subject, which still more deeply concern me. I have been deceived, it is said, or have wished to deceive others, respecting the result of some of my experiments; which were consequently not only cruel, but injurious to the cause of science, which they professed to serve.

In the present edition, a few observations are added, and some change is made in the arrangement. The work is divided into three parts, and in the eleventh chapter of the second part, the experiments and observations relating to the agency of galvanism in the animal economy, which by the former arrangement were intermixed with several other parts of the Inquiry, are presented at one view.

PREFACE

TO THE

FIRST EDITION.

THE obscurity of the nature of Internal Diseases, of which Physicians have always complained, seems to arise from several causes; the difficulty in those diseases of referring the painful feeling to the seat of the injury, proceeding from the indistinctness with which we refer to internal parts, and other parts sympathizing with the part affected; the deficiency of our knowledge of morbid anatomy, in consequence of which we are not always enabled from the train of symptoms to infer the derangement of structure; our ignorance of the function of many internal parts, and where we have a knowledge of the function, our ignorance of the principle on which it depends. If such be the causes of the obscurity of the nature of Internal Diseases, we may easily perceive the objects which ought to be kept in view in our endeavours to obtain a more correct knowledge of them, and consequently of the means of cure adapted to them.

By the frequent inspection of dead bodies, we learn to connect particular trains of symptoms with the changes of structure which occasion them; for although the sensations of which the patient complains are often ill defined, and sometimes not referred to the seat of the morbid action, yet the same morbid action almost always produces nearly the same train of symptoms. Nothing appears more to have retarded the progress of medical knowledge, than the obstacles which have in all ages been opposed to the inspection of dead bodies. The importance of the information thus obtained, however, has been slowly reconciling the public mind to it; and we may with confidence anticipate the greatest improvements from the increasing frequency of this practice; without which we can no more acquire a knowledge of the diseased states of the body, than we can of its healthy state without the aid of anatomy.

But neither anatomy nor the inspection

of morbid bodies can teach us the nature of the functions. A knowledge of them can only be acquired by comparing the structure of the organs with the actions observed in them while their vital power remains. Some of these actions are the objects of simple observation in our own bodies and those of other animals. Anatomy, for example, teaches us the structure, position, and attachments of the muscles; observation readily points out their function, and by the lesion of this function we judge of the extent and degree of their morbid affections, and are consequently guided in the application of our remedies. But there are other and more important functions, which in the entire animal are hidden from our view. To ascertain their nature, experiments must be made on the living and newly-dead animal. We shall find that many parts retain their vital actions for a certain time after what we call death.

To the aversion to experiments on living animals, which every man must feel, we may, I think, in a great measure, ascribe the little progress which has been made in

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this essential branch of medicine. Something too must be ascribed to the obscurity of the subject. The internal functions of animals are of a nature so different from any thing which we are accustomed to see around us, that our previous experience gives us little assistance in attempting to trace their laws, and our progress is necessarily slow and difficult. Hence it appears to have been, that the earlier Physiologists, disgusted with the task placed before them, evaded it; and endeavoured by ingenious fictions to deceive their readers. It is now universally agreed that if any progress can be made in Physiology, it is not by the wanderings of fancy, but by patience and by labour. We are amused with the reveries of Stahl, but for instruction we look to the experiments of Haller. And to similar experiments we must look for all the information we can obtain on this subject. The hope of adding something to our knowledge of the vital functions, and thus improving the treatment of their diseases, induced the author to undertake the following Inquiry.

From the foregoing view of the subject

it will be admitted, I think, that few writers have stronger claims on the indulgence of the public than the Physiologist, provided his endeavours are rationally directed. The knowledge which it is his aim to acquire and communicate, is of the most important kind, while his means of information are always laborious, and often of a painful nature. These claims are increased by the circumstances in which he is usually placed. No person is fitted for physiological inquiries, who has not obtained a competent knowledge of the different branches of medicine. This knowledge is acquired with so much difficulty, and depends so much on actual observation, that few who do not practise medicine as a profession ever acquire it. The Physiologist, therefore, generally pursues his inquiries amidst anxious and fatiguing engagements of a different kind, and of such a nature that all others must give place to them. I do not mention these circumstances as affording any apology for inaccuracy in points of consequence, because a writer owes it to the public to withhold his communications till he thinks himself assured of their accuracy; but they may, I hope, be admitted as an apology for less important errors. The errors of the following Inquiry are not those of precipitation. It is above fifteen years since some of the experiments which I am about to relate, and many connected with them, were made. None have been made within the last year, during which I have employed the time I could allot to such engagements in arranging my experiments, comparing them together, and endeavouring to guard against hasty inferences, which it is difficult to do at the time the experiments are made.

I have endeavoured as much as possible to avoid experiments on living animals. Most of those related in the following Inquiry were made on the newly-dead animal; and it will appear, I think, from what I am about to lay before the reader, that for many experiments, for which the living animal has been thought necessary, the newly-dead animal may be used with equal, and sometimes with greater advantage. When it was necessary to use the living animal, I uniformly observed the following rules:—to destroy the sensibility previous to the experiment, when this could be done without influencing the result; when several animals were equally fit for the experiment, to choose the one which would suffer least from it; when there were several ways of performing the experiment, to choose the way which would occasion least suffering; if the experiment was necessarily fatal, to destroy the animal as soon as the purpose in view was answered; and to take such precautions as rendered as few repetitions as possible requisite.

I have been much indebted, in making the experiments, to the kind assistance of several gentlemen; particularly Dr. Hastings*, of Worcester; Mr. Sheppard, Surgeon, in Worcester, and Mr. Herbert Cole, House Surgeon to the Worcester Infirmary. I shall frequently have occasion to mention those gentlemen.

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* Lately Mr. Hastings.

AN

EXPERIMENTAL INQUIRY,

&c.

THE following Treatise is divided into three Parts. In the first Part I shall make the reader acquainted with the state of our knowledge respecting the principle on which the action of the heart and blood vessels depends, and the relation which subsists between them and the nervous system, at the time my experiments were begun; as on this, all our knowledge of the vital functions more or less immediately depends. In the second, I shall relate these experiments, and point out the inferences to which they seem to lead; and in the last, consider their application to explain the nature, and improve the treatment of diseases.

PART I.

Of the State of our Knowledge respecting the Principle on which the Action of the Heart and Blood Vessels depends, and the Relation which subsists between them and the Nervous System.

The object of this Part cannot I think be better accomplished than by laying before the reader a translation of the Report of the Committee of the Institute of France, on the Experiments of M. le Gallois, and such observations on it as it appears to demand.

CHAP. I.

The Report made to the Class of Physical and Mathematical Sciences of the Institute of France on the work of M. le Gallois, entitled Expériences sur le Principe de la vie, notamment sur celui des mouvemens du cœur et sur le siége de ce Principe.

The Class having charged M. de Humboldt, M. Hallé and myself*, to make a report to it on the Memoir read at a meeting of the 3d of June last, by M. le Gallois, Doctor of Medicine, respecting the nature of the power of the heart, and whence it derives its power*, we are about to present to it a detail which will, perhaps, be as long as the Memoir itself, because without the necessary details and explanations it would be impossible to appreciate all the merit of this excellent work.

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* M. Percy.

It was not till after the circulation of the blood was discovered by Harvey, early in the seventeenth century, that Physiologists turned their attention to the cause and mechanism of the movements of the heart, which have, since that time, given rise to so many different systems.

We shall not speak of those of Descartes †, of Sylvius de le Boe ‡, of Borelli §. They are very absurd, and serve only to prove how unfortunate were the first attempts to explain one of the most important functions of the animal economy. We shall begin with the distinction which Willis first pointed out between the

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[*] "Concernant le principe des forces du cœur, et le siége de ce principe."

[†] L'homme de René Descartes, et la formation du fœtus avec les remarques de Louis Laforgue, Paris, 1677, p. 4 and 106.

[‡] Francisci De la Boe Sylvii Opera Medica Genevæ, 1681, p. 5, 27, 28, 33, 475.

[§] Joh. Alph. Borelli de motu animalium. Hagæ Comitum, 1743, p. 89—92.

nerves destined for the voluntary, and those for the involuntary motions. He placed the origin of the latter in the cerebellum, of the former in the brain, properly so called. He taught that the motions of the heart, and other vital organs, experience no interruption, because the cerebellum is in a state of constant activity; but that the organs of voluntary motion, on the contrary, require repose, because the brain acts only by intervals*. This distinction of Willis was very generally admitted till the middle of the last century. It was chiefly with a view to it that the division of the eighth pair of nerves, from which it was maintained that almost all the nerves of the heart proceed, was performed in different countries. The object was to prove that it is from the cerebellum that the heart derives all its power, and it was alleged that the animal died in this experiment, in consequence of the communication between these organs being interrupted. But, besides that it dies too slowly to permit us to ascribe its death to this cause, it has been proved in later times by several Philosophers, and particularly by M. le Gallois, in a memoir which the Class ordered to be inserted in the transactions of learned correspondents, that death here proceeds from quite a different cause. It has sometimes happened, indeed, that animals have died almost suddenly after the division of the nerves in question, and the partisans of Willis have not failed to lay much stress on this circumstance, of which their adversaries could give no satisfactory explanation. But M. le Gallois has demonstrated in the memoir to which we have just alluded, that sudden death in this case only happens in certain kinds of animals, and in these only when they are very young, and that it is the effect of suffocation*, more or less complete from the shutting of the glottis. There is nothing then in these facts in favour of Willis; to which we may add, that the eighth pair of nerves does not arise from the cerebellum, and that most of the nerves of the heart do not belong to this pair.

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* Tho. Willis opera omnia, edente Ger. Balsio Amstelodami, 1682, Tom. 1, de cerebri anatome, cap. xv. p. 50.

Boerhaave was of the same opinion with Willis, but besides the nervous influence, he admitted two other causes of the motions of the heart; the action of the blood of the coronary arteries on its fibres, and of the venous blood on the surface of its cavities. According to him the concurrence of these three causes produces the systole, and the simultaneous interruption of their action in consequence of the systole gives rise to the diastole, during which their action is renewed *. But this explanation, with the exception of what regards the stimulus of the blood on the internal surface of the heart, is contradicted by fact, which has not prevented its reception in the schools, with another error that has made no less noise.

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* Asphixie. This I translate suffocation, because we use the term Asphixia in a very different sense. Culleni Synopsis Nos. Method. Gen. 44.

We allude to Stahl, and his soul or Archæus, which, regulating all the movements of the living body, subjecting them to the will, or rendering them independent of it, according as they are merely useful, or absolutely necessary to life, presides above all over those of the heart, and, through the influence of the nerves, insures their continuance; a species of reverie which is inconsistent with all the true principles of Physiology.

After all, where would the Stahlians place this simple and indivisible being? In the brain without doubt. But then, how does it happen that an animal may live, and the motion of its heart continue after it is decapitated? Would they place it in the heart itself? But all animals, and especially those of cold blood, live a longer or shorter time after the heart is cut out †.

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* Her. Boerhaave Instit. Medicæ, § 409.—Vanswieten in Aphorismos, &c. Lugduni Batav. 1745, Tom. 2, p. 18.

† For an exposition and refutation of this system, see Haller's Element. Physiolog. Tom. 1, p. 480—8, and Tom. 4, p. 517—34.

Other writers, such as Abraham Eus *, Stœhelin †, &c., have also endeavoured to explain the motions of the heart; but their systems, almost as soon forgotten as conceived, do not deserve to detain us.

Those of Boerhaave and Stahl reigned almost alone, when in 1752 Haller published his experiments on irritability. These experiments and those of his followers tend to prove, that the contractile power belongs essentially to the muscular fibre. That property which Haller sometimes speaks of under the name of *vis insita*, sometimes after Glisson, under that of irritability, is the source of all the motions which take place in the animal; but it cannot produce them except some cause, some stimulus determines it to act. Thus all muscular motion implies two things, the irritability which produces the contraction of the muscle, and the *stimulus* which determines the irritability to act. The irritability is every where the same. It only varies in intensity in the different muscles; but it does not obey the same *stimuli* in all the muscles. The nervous power is the natural stimulus to all those which are under the influence of the will; and it is by exciting or suspending the action of that power on the irritability of such or such muscles, that the will causes any particular part to act or to be at rest. It is not thus with the muscles of involuntary motion ; these are affected by stimuli of different kinds, which are appropriated to their different functions, and altogether different from the nervous power. It is the blood which is the natural stimulus of the irritability of the heart : alimentary substances, of that of the intestinal canal, &c.

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* Dissertatio Physiol. de causa vices cordis alternas producente. Ludg. Batav. 1745.

† Dissertatio de pulsibus. Basileæ, 1749.

We easily deduce from these principles the explanation of the leading circumstances which we observe in the motions of the heart. Thus its motions are involuntary, because they are independent of the nervous system ; they take place without interruption during life, because the irritability which produces them belongs essentially to the fibres of the heart, and the blood which excites them is constantly supplied to this organ by the veins as it is carried off by the arteries. The systole and diastole succeed each other alternately and regularly, because the stimulus of the blood always occasions the former both in the auricles and ventricles, and the systole itself, by expelling the stimulus, occasions the diastole, which renews the systole by allowing access to new blood.

Such is a summary view of the celebrated

Hallerian theory of irritability. That theory was not contrived in the closet like the others of which we have spoken: it was founded, as we have said, on experiments made by Haller himself, and by the most distinguished of his scholars, who then occupied, or have since occupied, the first rank among the Anatomists and Physicians of the last age. The inferences from these experiments, which were repeated throughout Europe, found almost every where supporters; but they found also some opponents of the greatest reputation. The principal cause of this difference of opinion, and that respecting which authors have not yet been able to come to any agreement, is the question, whether the motions of the heart are really independent of the nervous system?

We may reduce to three heads the facts by which the school of Haller has supported the affirmative. 1st. If we interrupt all communication between the heart and the brain, *the only source of nervous power*, by dividing the nerves which go to the heart, the spinal marrow in the neck, or even by decapitation, the motions of the heart continue as before. 2d. If we cut out the heart of a living animal, and place it on a table, it continues to beat, and sometimes for a long time. M. de Humboldt has shewn that it beats more strongly, and for a longer time, when it is suspended. 3d.

We always produce convulsions, even for some time after death, in the muscles of voluntary motion, by irritating their nerves, either mechanically or in any other way. On the contrary, the irritation of the cardiac nerves occasions no change in the motions of the heart, nor re-calls them when they have ceased. The same observation is true of the Medulla oblongata and spinal marrow, the irritation of which occasions strong general convulsions, but produces no effect upon the heart.

These facts are correct, except perhaps those of the third head, respecting which there is some difference of opinion. For in admitting them, the adversaries of irritability have asked, Why, if the nervous power has no action on the heart, is this organ supplied with nerves, and why is it so evidently subjected to the influence of the passions? Haller never gave any satisfactory explanation of these objections, but every thing proves that he felt all their force. When we read with attention all that he has said of the motions of the heart, in his dissertations on irritability*, and above all in his great work on Physiology†, we are struck with the contradictions which we meet with in them, and which makes the perusal of them fatiguing.

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* Mémoires sur la nature sensible et irritable des parties, etc. Lausanne, 1756.—Opera Minora, Tom. 1.

† Element. Physiol. lib. iv. sect. 5, et lib. xi. sect. 3.

Through all of them his great object is to prove, that the motions of the heart are independent of the nervous system. All the facts, all the experiments, all the observations which he brings forward, tend to this end; and yet he seems to admit in several places that the nerves possess an influence over the heart. It is true that it is with an air of doubt that he admits it, and confines himself to saying, that it is possible, that it is not unlikely, that the heart derives a power of motion from the nerves*. These contradictions with which several justly celebrated writers have reproached him, amongst others MM. Prochaska†, Behrends‡, Ernest Platner§, &c., proceed evidently from his not being able to reconcile the results of experiments with the influence of the nervous power over the motions of the heart; and, in rejecting this influence, finding it impossible to explain the use of the cardiac nerves and the effect of the passions on the heart. Here is the great difficulty in the controversy of which we speak. Those who, like Fontana, formally reject all intervention of the nervous influence, have been forced to admit that the nerves, destined to convey to every other part life, feeling, and motion, have no known use in the heart*.

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* Element Physiol. lib. iv. sect. 5, p. 493, et alibi passim.

† Opera Minora, Viennæ, 1800, Tom. II. p. 90.

‡ Vol. 3, p. 4, of the Collection of Ludwig, entitled *Scriptores neurolog. minores selecti*, Lipsiæ, 1791—5. Four volumes, in 4to.

§ Vol. 2, p. 266, of the same Collection.

Such consequences evidently disclose the insufficiency of the theory of Haller, and several of his followers have acknowledged the necessity of some modification of it, and admit the nervous power to be one of the principles on which irritability depends. They are thus enabled to assign a use to the nerves of the heart, and to explain the influence of the passions on this organ. But when they have attempted to explain why the interruption of all communication between the brain and the heart does not stop the motions of the latter, they have been obliged to abandon the generally received opinion, which regards the brain as the only centre and source of nervous power, and have admitted, without any *direct proofs*, that that power is generated throughout the whole extent of the nervous system, even in the smallest nerves, and that it can exist for a certain time in the nerves of any part independently of the brain. Among the authors of this opinion, the learned Professor Prochaska is one of those who has given the best account of it*. But when he applies it to the motions of the heart, and attempts to explain why they are independent of the will and yet influenced by the passions, his opinion appears undecided. He has recourse to the ganglions, and hesitates what function to ascribe to them. Sometimes he considers them as knots, as ligatures, so tight as to intercept all communication between the heart and *Sensorium Commune*, in the calm and peaceful state of the system, but not sufficient to prevent the Sensorium re-acting more or less powerfully on the heart in the agitation of the passions†. Sometimes he seems to believe that the interception is complete and constant, and that it is by the nerves of the eighth pair that the passions affect the heart ‡; and he seems to adopt the opinion of Winslow §, renewed by Winterl ||, Johnstone ¶, Unzer **, Lecat ††, Peffinger ‡‡, &c.,

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* Memoires sur les parties sensibl. et irritab. Tom. 3. p. 234. See also Caldani, ib. p. 471, and Le Traitê sur le venin de la vipère, Tom. II. p. 169—171.

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* Commentatio de functionibus systematis nervosi, published in the third fasciculus of the Annotationes Academ. of this writer, and re-printed at Vienna in his Opera Minora, in 1800.

† Opera Minora, Tom. 2, p. 165.

‡ Ibid, p. 167.

§ Exposit. Anatom. Traité des Nerfs, § 364.

|| Nov. Inflam. Theoria, Viennæ, 1767, cap. 5, p. 154.

¶ Essay on the Use of the Ganglions, 1771.

** Unzer quoted by Prochaska, oper. minor. Tom. 2, p. 169.

†† Traité de l'existence de la nature et des propriétés du fluide nerveux, Berlin, 1765, p. 225.

‡‡ De structura nervorum, Argentorati, 1782, Sect. 1, § 34, inserted in the Collection of Ludwig, vol. 1.

that the ganglions are so many small brains. He admits at the same time that the nerves of feeling are distinct from those of motion, so that the heart cannot contract except when the impression of the *stimulus* on its cavities is transmitted to the ganglions by the nerves of feeling, and reflected on its fibres by the nerves of motion*. But besides that this opinion, even by the author's confession, is only a conjecture, it supposes on the one hand, that the circulation would continue after the destruction of the spinal marrow; and, on the other, that the heart would cease to beat at the moment when its communication with the ganglions and the Plexus is interrupted. Now both these suppositions are contradicted by facts.

These fruitless attempts to modify the theory of irritability by the intervention of the nervous power, have only increased the zeal of some authors to maintain that theory in its original purity, and as the use of the nerves of the heart was among the most embarrassing objections to it, M. Sœmmerring, one of the most profound Anatomists of Germany, and Behrends, one of his most distinguished scholars, maintained, in 1792, that the heart has no nerves, and that all those which appear to enter it are expended on the coats of the coronary arteries, without the fibres of the heart receiving a single thread* ; an opinion which far from removing all the difficulties, only renders the influence of the passions on the motions of the heart more inexplicable. These two authors maintain that the cardiac nerves support and increase the irritability of the coronary arteries ; but the existence of irritability in the arteries is still doubtful, and were it demonstrated, it would be very strange if irritability depended on the nervous influence in the arteries ; and in the heart, the most irritable of all the organs, it were wholly independent of this influence.

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* Opera Minor. Tom. II. p. 169.

Science, however, has cause to rejoice at the groundless doubts proposed by M. Behrends respecting the cardiac nerves, since they have induced the learned Scarpa to take part in the dispute, and have procured for us his excellent work on the nerves of the heart†. M. Scarpa proves in that work that the nerves of the heart are as numerous, and are distributed in the same way, as in other muscles. He admits with M. Prochaska, that sensibility and irritability are essentially united, and that the nervous influence is generated throughout the whole extent of the nerves ; but he does not admit that the ganglions are so many little brains*. He seems to believe that the nervous influence, such as it exists in all the nerves, is of itself sufficient for the exercise of the different functions, and that it only wants the stimulus which excites it to action. That the stimulus of the muscles of voluntary motion comes from the brain, and that in ordinary states the blood is the stimulus of the heart: but that in vivid emotion the brain also becomes a stimulus to this organ†.

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* Behrends Dissertatio qua demonstratur cor nervis carere, Moguntiæ, 1792, inserted in the third vol. of the Collection of Ludwig.

† Tab. neurolog. ad illust. hist. anat. cardiacorum nervorum, &c. Ticini, 1794.

According to this opinion the heart ought to beat in the same manner, and with the same force, after decapitation, after the destruction of the spinal marrow, and after it is removed from the body. M. Scarpa himself compares the beating of the heart in apoplexy to that which we observe when it no longer communicates either with the brain or spinal marrow‡. But we shall see in the sequel that it is very different. We must not omit a very important remark of this author, and which it is surprising was not sooner made: it respects the insensibility of the heart when we irritate the spinal marrow and the cardiac nerves. M. Scarpa observes, that that insensibility of which so much has been said, and which has been regarded as a demonstrative proof that the motions of the heart do not depend on the nerves, proves only that the nerves of the heart are not of the same kind with those of the muscles of voluntary motion, and that the nervous power does not in them obey the same laws[*]. This reflection is without doubt very judicious, and it is by an error of experimental logic that we are surprised not to obtain the same effects from the irritation of two orders of nerves wholly different.

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* Tab. neurolog. ad illust. hist. anat. cardiacorum nervorum, &c. § 30. Ticini, 1794.

† Ibid, § 22, 24, 25, 26, 27, 29.

‡ Ibid, § 25.

The work of M. Scarpa did not induce Dr. Sœmmerring[†] to change his opinion, nor prevent Bichat from denying that the nervous power has any share in the motions of the heart[‡]. This last writer maintains the existence of an animal and organic life, distinct from each other, and of a nervous system for each of these lives. The system of the ganglions, which he regards in the same point of view with the authors above quoted, as small brains, belong to the organic life, and the cerebral system to the animal life[§]. To be consistent with himself, Bichat should have admitted, like M. Prochaska, that the heart, the centre of organic life, derives from the ganglions the principle of its motions; but he has not done so. It is chiefly the galvanic experiments which has brought him into this inconsistency, because he had attempted in vain to produce contractions in the heart by galvanising the cardiac nerves; experiments on which M. Sœmmerring and Behrends had also endeavoured to support their opinion. These experiments may always succeed, as one of us found in 1797[*]. and three years before was found by Mr. Fowler[†].

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[*] Tab. neurolog. ad illust. hist. anat. cardiacorum nervorum, &c. § 20.

[†] Th. Sœmmerring de corporis humani fabrica Trajecti ad Mænum, 1796, tom. III. p. 30, 43, 46, 50, et ibid., 1800, Tom. V. p. 43.

[‡] Bichat. Recherch. Phys. sur la vie et la mort, Paris, 1800, Part II. Art. 11, § 1.

[§] Ibid., Part I. Art. 6, § 4. Ibid., Art. 1, § 2,

Such is a short but faithful account of the principal systems, by means of which authors have, since the discovery of the circulation of the blood to this day, attempted to explain the motions of the heart. On taking a general view of them we remark, that in all those invented before Haller[‡], the nervous power is considered, in one way or other, as one of the conditions essential to the production of the motions of the heart; and it is always and only in the brain that they place the seat of it. The cardiac nerves, therefore had a determined use in all these systems, and one could easily understand why the heart is subject to the empire of the passions; but it was impossible to explain why the circulation continues in acephalous animals, and why in experiments on animals, the interruption of all communication between the brain and the heart does not stop the motions of the latter. Since Haller, irritability has been the basis of all these systems. In regarding that property as essential to the fibre and independent of the nervous influence, the circulation in acephalous animals, and the different phenomena observed in the experiments alluded to, present nothing that is not easily understood; but the use of the nerves of the heart and the influence of the passions on that organ become inexplicable. The necessity of removing these difficulties has produced two parties among the supporters of irritability. The one, zealous favourers of the doctrine of pure irritability, called to their aid the most improbable hypotheses, and all their efforts have only served to prove how difficult it is to support the cause they espouse. The other confounded the nervous power with irritability, which they consider as one of the functions of that power; but they have been obliged to admit, either with respect to the seat, or the manner of existence, of the nervous power, conditions, which, by their own confession, are far from being demonstrated, respecting which they are not agreed, and which, in the application they make of them to the motions of the heart, either do not wholly remove the old difficulties or create new ones.

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[*] M. de Humboldt. Expériences sur l'irritation de la fibre nerveuse et musculaire, publiées en 1797, et traduites en Français deux ans après, Tom. I. Chap. 9.

[†] Experiments on Animal Electricity, by Richard Fowler, 1794.

[‡] Also in those of Ens, of Stœhelin, and others of whom we have spoken.

One may easily see why so little progress has been made in this great and long disputed question. If we examine all that has been said on the subject since the days of Haller, we shall find, that both sides have constantly brought forward nearly the same facts, the same experiments, and the same reasonings. The only new experiments are the applications of galvanism to stimulate the cardiac nerves; and they are only new in appearance, for from the time of Haller electricity has been employed with the same view*. It is evident that science had nothing to expect from our pursuing a path trodden for nearly sixty years by so many celebrated men. It was necessary to open new roads; it was necessary to find or invent new modes of interrogating nature. It was, above all, necessary to introduce into physiological experiments, that precision and severe logic to which other branches of physical science have, in our days, owed so great progress. It is this which the author of the memoir before us has done.

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* See, amongst others, Mém. sur les parties sensib. et irritab. Tom. III. p. 214.

It was not the original object of M. le Gallois to explore the cause of the motions of the heart. He had adopted the theory of Haller on this subject, when experiments undertaken with other views led him to the singular conclusion, that it was impossible for him to understand his own experiments, without determining whether the nervous power influences the motions of the heart; and if so, in what way it has this effect. To make his work better understood, we shall relate on what occasion, and by what chain of facts and reasonings he was led to engage in this inquiry.

A peculiar case of labour some years ago excited in him a wish to know how long a full-grown fœtus can live without breathing, after all communication between it and the mother has ceased. That question, curious in itself, and of the first importance in the practice of midwifery and medical jurisprudence, had hardly been touched upon by authors. M. le Gallois undertook to resolve it by direct experiments on animals; and that the solution might be generally applicable, and extend to as many cases as possible, he placed the fœtus of animals in various situations similar to those in which the human fœtus is occasionally placed, when it ceases to communicate with the mother. Among these there is one which occurs too often, namely, the fœtus suffering decollation from artificial delivery by the feet. The author wished to know what happens to the fœtus in this case, whether it perishes at the instant of decollation, and how death takes place. He found that the trunk retains its life, and that if hemorrhage be prevented, by throwing a ligature round the vessels of the neck, it dies in the same time and with the same symptoms as if, without taking off the head, respiration had been interrupted; and what completely demonstrated to him that a decapitated animal is in fact suffocated, is, that we may at pleasure prolong its existence by inflating the lungs to supply the place of natural respiration.

M. le Gallois concluded from these facts, that decollation proves fatal by destroying the motions of inspiration, and that consequently the power on which these motions depend is in the brain; but that that on which the life of the trunk depends is in the trunk itself. Endeavouring to ascertain the precise seat of each of these powers, he found that that on which the motions of inspiration depend resides in that part of the medulla oblongata from which the eighth pair of nerves take their rise; and that on which the life of the trunk depends, in the spinal marrow. It is not by all the spinal marrow that every part of the body is animated, but only by that portion from which it receives its nerves; so that in destroying any particular part of the spinal marrow, we only destroy life in those parts of the body which correspond to that part. Besides, if we interrupt the circulation in any particular part of the spinal marrow, life is weakened, and soon extinguished in all the parts which receive nerves from it. There are, therefore, two ways of destroying life in any part of an animal; the one destroying that part of the spinal marrow from which it receives its nerves, the other interrupting the circulation in this part of the spinal marrow.

It hence results, that two conditions are necessary to preserve the life of any part of the body, viz., the integrity of the corresponding part of the spinal marrow, and the circulation of the blood, and consequently that we may preserve life in any part of an animal as long as we can preserve in it these two conditions. We may, for example, preserve the life of the anterior parts after that of the posterior parts is destroyed, by destroying the corresponding portion of the spinal marrow, or *vice versâ*.

M. le Gallois, whose constant practice was to seek in direct experiments a confirmation of the consequences which he deduced from preceding ones, wished to know if in fact it is possible to make any particular part live after the others are dead. In a rabbit, twenty days old, he destroyed all the lumbar portion of the spinal marrow. This operation occasioning no immediate injury to the rest of the spinal marrow, and, according to the theory of Haller, the circulation not being affected by it, he had every reason to expect, reasoning from the preceding experiments, that the animal would have lived for a considerable length of time, and that it would only have died in consequence of the symptoms produced by so severe an injury; but the respiration ceased in a minute or two, and in less than four minutes it shewed no sign of life. This experiment was repeated several times with the same result, nor was it possible to prevent it. Thus it was proved, that a rabbit of twenty days old cannot survive the loss of the lumbar portion of the spinal marrow; which appeared the more surprising, because rabbits of this age continue to live very well after decapitation, that is, after the total loss of the brain. This fact, which the author could not reconcile with his preceding experiments, led him to discover, that the power on which the action of the heart depends *(le principe des forces du cœur)* resides in the spinal marrow.

M. le Gallois then ascertained that the destruction of either the dorsal or cervical portion of the spinal marrow was fatal to rabbits of twenty days old, even in a shorter time than that of the lumbar portion, in about two minutes. He found that the same experiments repeated on rabbits of different ages did not give the same results. In general the destruction of the lumbar portion was not suddenly fatal to rabbits under ten days old, and some at the age of fifteen days survived it. Beyond twenty days old the effect is the same as at this age. Very young rabbits continued to live after the destruction either of the dorsal or cervical portion, but for a shorter time; and in a smaller number of cases, after the destruction of the latter than after that of the dorsal portion. None after the age of fifteen days survived the destruction of either.

In all those partial destructions of the spinal marrow, even where the death is sudden, it is instantaneous only in the parts which receive their nerves from the destroyed part, and only extends to the rest of the body at the end of a certain time; but this time is fixed, and no means can prolong it. It is the same in animals of the same kind and of the same age; and the longer, the nearer the animal is to the time of its birth. For example, when the cervical part of the spinal marrow is destroyed in rabbits, life is instantly lost in the whole of the neck ; but it continues in the head, as appears from the gaspings it excites ; it continues also in the parts below the shoulder, as the continuance of their feeling and voluntary motion shews. In the first day after birth, the gaspings continue about twenty minutes, the sensibility and motion of the rest of the body fifteen minutes. At the age of fifteen days, the duration of the gaspings does not exceed three minutes ; that of sensibility and motion two and a half. In fine, at the age of thirty days the gaspings cease in a minute and a half, and the sensibility in a minute. After the destruction of the dorsal portion of the spinal marrow, it is the chest and not the neck which is instantly struck with death. In other respects the phenomena and their duration are the same. If the three portions of the spinal marrow are destroyed at once, the gaspings, the only signs of life which then remain, have still, at the different ages, the durations just pointed out.

The author, who had so often decapitated rabbits of different ages, had always remarked, that the head, separated from the body, continued to gasp during a time determined by the age. This time was evidently the same as after the destruction of the spinal marrow. Now it is evident that after decapitation there can be no longer any circulation in the head, and that the gaspings which take place in that case can only continue for the time during which life may exist in the brain, after the total ceasing of the circulation. This was the first indication which M. le Gallois had, that when the partial destruction of the spinal marrow occasions death throughout all the rest of the body, it is because it suddenly arrests the circulation. To assure himself of this, he cut out the heart at the base of the great vessels, in rabbits of every fifth day old from birth to the age of a month: and having noted with care the duration of the different signs of life from the moment at which the circulation was thus stopped, he found, that their duration was precisely the same as after the destruction of the spinal marrow. He might have considered this coincidence as sufficient to decide the question; but he wished to ascertain in a more direct manner, if the circulation actually ceases at the moment the spinal marrow is destroyed. The absence of hemorrhagy and the emptiness of the arteries were the most evident signs that he could have of the circulation having ceased; and he found, in fact, that soon after the above operation, the carotids were found empty, and the amputation of the limbs occasioned no hemorrhagy, though performed near to the trunk, and before life was extinct in the parts of which the spinal marrow had not been destroyed. In a word, all the signs which shew the state of the circulation demonstrated to him, that when the destruction of any part of the spinal marrow suddenly occasions death in the rest of the body, it is by stopping this function; and this effect takes place, not because the motion of the heart immediately ceases, but because it is no longer capable of throwing the blood even into the carotids.

Hence it follows, that it is in the spinal marrow that the power on which the motion of the heart depends resides, and in the whole of it, since the destruction of any one of its three portions is capable of stopping the circulation. It also follows, that each portion of the spinal marrow influences life in two different ways; by the one it is essential to the existence of life in the parts which receive nerves from it; by the other, it preserves it throughout the body in general, by contributing to furnish to the organs which receive nerves from the great sympathetic, and particularly to the heart, the life and power, *(le principe de force et de vie)* necessary to the performance of their functions.

Thus we see, that to make the anterior or posterior parts of an animal live after killing the rest of the body, by destroying the corresponding parts of the spinal marrow, we must prevent the destruction of these parts from stopping the circulation. Now this is easily done by diminishing the sum of the forces, which the heart must impart for the support of the circulation, in proportion as we diminish the power which it receives from the spinal marrow. It is sufficient for this purpose to diminish by ligatures, thrown round the arteries, the extent of the parts to which the heart sends the blood. We have seen, for example, that the destruction of the lumbar part of the spinal marrow is quickly fatal to rabbits at or beyond the age of twenty days; but this is not the case if we previously throw a ligature round the ventral aorta between the cœliac and anterior mesenteric arteries.

The application of this principle to other parts of the body leads to the singular conclusion, that in order to maintain life in rabbits of a certain age, after the destruction of the cervical part of the spinal marrow, we must previously cut off the head. They certainly die if this part of the spinal marrow is destroyed without previous decapitation. This fact ceases to surprise, when we reflect that by decapitation, we lessen by the head the extent of the circulation, and that by that means the heart having need of less force to support the circulation, we may enfeeble it by the destruction of the cervical part of the spinal marrow, without destroying the circulation.

One may easily conceive that any other operation capable of suspending or considerably enfeebling the circulation in any part of an animal may produce a similar effect; and enable us in like manner, to destroy such a portion of the spinal marrow, as would have been fatal without this previous operation. This is what happens in the partial destruction of the spinal marrow itself. It has two effects on the circulation; by the one it enfeebles it, generally by depriving the heart of that share of its power which it receives from the part of the spinal marrow that has been destroyed; by the other, without wholly destroying the circulation in the parts which are thus deprived of life, it in a great degree lessens it in a way in some measure similar to the effect of ligatures thrown round the arteries of these parts. But this effect is not remarked till a few minutes after the destruction of the spinal marrow. Thus it is the destruction of the first part of the spinal marrow which enables us to destroy a second; this a third, and so on. For example, when, by decapitating a rabbit, we are enabled to destroy the cervical part of the spinal marrow, the destruction of that part in a certain number of minutes enables us to destroy the fourth part of the dorsal portion of the spinal marrow, and thus by continuing to destroy parts of similar extent, by intervals, we may at length destroy the whole of this portion of the spinal marrow without stopping the circulation, which is then supported by the lumbar portion only.

We may collect from what has just been said, that in rabbits, each portion of the spinal marrow bestows on the heart power sufficient to support the circulation in all those parts which correspond to that portion, and consequently, that in cutting a rabbit transversely, it would be possible to make each portion live for an indefinite time, if the lungs and the heart, necessary for the formation and circulation of arterial blood, could make part of it. But they can only make part of the chest, and one may very well maintain the life of the chest alone and insulated, after having cut off both the anterior and posterior parts, and prevented hemorrhagy by proper ligatures, and that even in rabbits thirty days old or more.

Such are the principal results of M. le Gallois' researches, results which arise one from the other, and mutually supporting each other, are founded on direct experiments, made with a precision hitherto unknown in Physiology. We are now going to relate such of those experiments as the author repeated in our presence. We devoted to these repetitions three meetings, each of several hours' duration; and, in order to avoid all precipitation, and to give us time to weigh the facts at leisure, we allowed a week to intervene between the meetings.

Experiments repeated before the Committee of the Institute.

We shall divide them into two parts; the first will comprehend those which tend to prove that the origin of all the motions of inspiration reside in that part of the medulla oblongata which gives rise to the eighth pair of nerves. In the second, we shall relate those whose object is to prove that the heart derives its power from the spinal marrow.

1*st. Experiments relating to the power on which the motions of inspiration depend.*

The author, in a rabbit of five days old, detached the larynx from the os hyoides, and exposed the glottis that we might observe its movements; after which he opened the head, and first extracted the cerebrum and then the cerebellum. After these operations the inspirations continued; they were each characterized by four simultaneous movements, namely, a gasping, the opening of the glottis, the elevation of the ribs, and the contraction of the diaphragm; these four movements having been observed, and found to continue for a certain time, according to the age of the animal, the author extracted the medulla oblongata, and in a moment these movements ceased altogether. The portion of the medulla oblongata which was extracted extended to the occipital hole, and included the origin of the eighth pair of nerves.

The same experiment was repeated on another rabbit of the same age, with this difference, that after the extraction of the cerebrum and cerebellum, instead of removing so large a portion of the medulla oblongata all at once, it was extracted successively, by portions of about the thickness of three millimetres. The four motions of inspiration continued after the extraction of the three first slices, but ceased immediately after that of the fourth; we found that the third slice terminated at the posterior part, and very near to the pons varolii, and that the fourth embraced the origin of the nerves of the eighth pair.

This experiment, repeated on other rabbits, constantly gave the same result.

The same experiment was made on a cat five weeks old, except that before the medulla oblongata was removed by slices the two recurrent nerves were divided. The glottis immediately closed and remained immoveable, but the three other motions, namely, the gaspings, the elevation of the ribs, and the contractions of the diaphragm continued, and only ceased at the moment when that portion of the medulla oblongata, in which the eighth pair of nerves originate, was removed.

It is evident that if in place of destroying that part from which all the motions of inspiration are derived, one only cuts off the communication between it and the organs which perform these motions, he will produce the same effect; that is to say, will stop those motions whose organs have no longer any communication with the part in question. This is what we have just seen happened in the cat, in which the division of the recurrent nerves stopped the motions of the glottis without stopping the other three motions. In order to suspend these it is sufficient to observe how their organs communicate with the medulla oblongata. Now it is clear that it is by the intercostal nerves, and consequently by the spinal marrow, that the medulla oblongata acts upon the muscles which raise the ribs, and that it is by the phrenic nerves, and consequently by the spinal marrow also, that it acts on the diaphragm. In dividing the spinal marrow about the last cervical vertebra, and below the origin of the phrenic nerves, one ought therefore to stop the motions of the ribs, but not those of the diaphragm: and in dividing the spinal marrow between the occiput and the origin of the phrenic nerves, we ought to destroy at once the motions of the ribs and those of the diaphragm, and this is in fact what happens. The author, after the motions of the thorax had been well observed in a rabbit about ten days old, divided the spinal marrow about the seventh cervical vertebra. Such of these motions as depend on the elevation of the ribs, immediately ceased, but the contraction of the diaphragm continued. He then divided the spinal marrow about the first cervical vertebra, and immediately the diaphragm ceased to contract. Lastly, he divided the eighth pair of nerves about the middle of the neck, and the motions of the glottis ceased. Thus of the four motions of inspiration there remained only the gaspings, which shewed that the medulla oblongata still preserved the power to produce all the motions, and that it only failed to produce the other three because it no longer had any communication with their organs. We ought to observe here, that several authors, amongst others Arnemann, before M. le Gallois, had observed that the division of the spinal marrow only stopped the motions of the diaphragm when it was made between the occiput and the origin of the phrenic nerves; but these authors regarded the brain as the only source of life, and of all the motions of the body. They thought, accordingly, that the division of the spinal marrow instantly paralyzed all parts of the body whose nerves arose from the spinal marrow below the part at which it was divided; and, therefore, that when the division was made near the occiput the diaphragm ceased to contract, because it partook of the paralysis of all the parts below the division. But M. le Gallois has demonstrated, that the division of the spinal marrow, made about the first or last cervical vertebra, only stops the motions of inspiration, and allows to remain throughout the body both feeling and voluntary motion. This distinction is essential. No person made it before him.

It is not only in warm-blooded animals that these experiments produce the results which we have described. To prove that these results belong to the general laws of the animal economy, and that the nervous power obeys the same laws in all vertebral animals, the author took a frog, and after having remarked that in these animals, which have neither ribs nor diaphragm, there are but two kinds of motions of inspiration, namely, those of the glottis, which opens in the form of a lozenge, and those of the throat, which is alternately raised and lowered, he cut off the anterior half of the brain, the two motions continued; he then destroyed about the half of that which remained; the motions still continued. In fine, he carried the destruction of the brain as far as the occipital hole, and the two motions instantly ceased. In another frog he divided the spinal marrow about the third vertebra; the motions of inspiration continued. In a third frog it was divided between the occiput and the first vertebra; the motion of the throat, which represents that of the diaphragm, immediately ceased. After these two last experiments the frogs were and remained alive, both in the head and the rest of the body; but they could not govern their motions, and in this respect were in the same state as the first frog, whose brain had been destroyed.

2d. *Experiments relative to the Principle on which the Power of the Heart depends.*

The author has already proved that life always continues for a certain time, even in warm-blooded animals, after the total ceasing of the circulation, and that the length of this time is influenced by the age of the animal. He opened the chest and cut out the heart of a rabbit of five or six days old; he did the same in another of ten days old. In the first the gaspings ceased in seven minutes, and the sensibility in four after the excision of the heart. In the second the gaspings lasted only four minutes and the sensibility only three. The cervical and a small portion of the dorsal part of the spinal marrow were then destroyed in another rabbit of the same litter with the last, and immediately afterwards the lungs were inflated; notwithstanding this assistance the gaspings ceased at the end of three minutes and a half, and the sensibility in a little more than two and a half; periods which coincide, we see, to nearly half a minute with those observed after the excision of the heart.

In order to prove that in this experiment it is really by stopping the circulation that the destruction of a part of the spinal marrow destroys the life of the rest of the body, the author divided the spinal marrow of a rabbit of the same age with the two last, near the occiput. After this division the carotid arteries were black, but round and full, and on the amputation of a limb, black blood flowed; having inflated the lungs, the carotids quickly regained a fine red colour, and blood also flowed from the limb of the same colour. These appearances left no doubt that the circulation continued after the division of the spinal marrow near the occiput. The author then destroyed in this rabbit the same portion of the spinal marrow as in the preceding. The carotids instantly became flaccid, and soon appeared empty and flat. The two thighs, amputated in less than two minutes after the destruction of the spinal marrow did not supply a drop of blood.

The destruction of the cervical part of the spinal marrow in several other rabbits, from twenty to thirty days old, gave precisely the same results, that is to say, the carotids soon appeared empty, and no blood flowed on the amputation of the limbs; and notwithstanding the most careful inflation of the lungs, the signs of life remained no longer than after the excision of the heart, according to the tables which M. le Gallois has given of the different ages in his paper.

The results were the same with respect to the emptiness of the carotid arteries, the absence of hemorrhagy, and the duration of life, after the destruction of the dorsal part of the spinal marrow.

The destruction of the lumbar part of the spinal marrow in rabbits of four and five weeks old gave similar results, with this only difference, that the circulation did not stop immediately, as after the destruction of the cervical and dorsal parts of the spinal marrow; but at the end of about two minutes, and in one case at the end of four minutes, which proves that the action of the lumbar part of the spinal marrow upon the heart, though evident and very great, is not so immediate as that of each of the other portions.

After having proved by these experiments that the circulation depends on all parts of the spinal marrow, the author shewed us that there is none of these portions which may not be destroyed with impunity, if we confine to a certain space the parts to which the heart sends the blood. After opening the belly of a rabbit six weeks old, he threw a ligature round the aorta, between the cæliac and anterior mesenteric arteries, after which he destroyed the whole of the lumbar part of the spinal marrow. This rabbit continued quite alive, supporting itself upon its fore legs, and holding up its head more than half an hour afterwards, when the Committee finished their sitting; while another rabbit, of nearly the same age, used for the sake of comparison, in which the lumbar portion of the spinal marrow had been destroyed without securing the aorta, died in less than two minutes.

M. le Gallois then made the experiment of destroying the cervical portion of the spinal marrow, the action of which upon the heart is more immediate, and still more considerable than the lumbar, in rabbits of five or six weeks old, without stopping the circulation. After having decapitated the animal with the ordinary precaution, he performed artificial inspiration during five minutes, at the end of which he destroyed the whole of the cervical part of the spinal marrow; he renewed the artificial inspiration immediately after, and the animal remained alive as long a time as it was judged proper to continue this operation. The same experiment was repeated with the same result on two other rabbits of the same age; in one of these, five minutes after having destroyed the cervical part of the spinal marrow, the author destroyed about one-third of the dorsal part of the spinal marrow, then five minutes after a second third, and the remaining part again in five minutes. The circulation and the life of the animal continued after the destruction of the two first third parts, and only ceased after that of the last. During the whole of the experiment artificial respiration had only been interrupted for the time necessary for the destruction of the spinal marrow.

These experiments led M. le Gallois to that much more difficult one, the object of which is to prove, that in limiting by ligatures the circulation to those parts which correspond to any particular portion of the spinal marrow, that portion gives to the heart power to support the circulation in those parts. He separated the upper and lower from the central parts in a rabbit of thirty days old, dividing it below about the first lumbar vertebra, and above about the second cervical vertebra, then by artificial respiration he supported life in the chest thus insulated. We do not describe the particulars of the operation, because the author has detailed them in his memoir. We shall confine ourselves to say, that the experiment succeeded perfectly, although an artery, which could not be secured, occasioned a considerable hemorrhagy, and risked the success of the experiment.

In fine, M. le Gallois produced partial death in the hinder parts of the body, in a rabbit of about twelve days old, by tying the aorta between the cæliac and anterior mesenteric arteries. At the end of twelve minutes the death of the parts appearing complete, he untied the artery, and life by degrees appeared in the whole of these parts, so that the animal was able to walk with ease. This partial resurrection proved that we might succeed in the same way with the whole body, if it were possible to re-establish the circulation after the extinction of life in the whole of the spinal marrow; but the experiments of the author demonstrate much better than had been done before him, why the renewal of life in the whole body is impossible.

The author has also made, in our presence, some experiments on Guinea Pigs; from which it appears, that in these animals the power of the heart equally depends on the spinal marrow, only it was necessary to destroy greater portions of it, in order to stop the circulation, than in rabbits of the same age.

We shall finish the account of the experiments which M. le Gallois repeated in our presence, by those on cold-blooded animals, the results of which are altogether in contradiction to those which the most zealous partisans of Haller, and among the rest Fontana[*], have obtained, and which have been so much valued. The author opened, on the one hand, the cranium, and on the other, the chest of a frog, and brought the heart into view; he then fixed the animal firmly[†], and while one of us observed the motions of the heart, measuring seconds with a watch, he destroyed the brain and the whole of the spinal marrow by a stilet, introduced by the opening in the cranium: in an instant the motions of the heart stopped, and were not renewed for several seconds, and the rate of their repetition never again became the same: they were more frequent than before the destruction of the spinal marrow. The same experiment repeated on five frogs constantly gave the same results; the motions of the heart were not suspended the same number of seconds in all, but the suspension was always very remarkable, as well as the change in the rate of beating. We may add, that the amputation of the thighs of frogs, after the destruction of the spinal marrow, occasioned no hemorrhagy; and salamanders, decapitated after a similar operation, in like manner lost no blood, while both in the one case and the other there had been hemorrhagy when the spinal marrow was allowed to remain entire. These experiments appear to us completely to confirm all the inferences which the author has deduced from them, and with which he finishes his memoir. To confine ourselves here to the principal points we shall say, that we regard as demonstrated,

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[*] Fontana. Mem. sur les parties sensib. et irritab. Tom. III. p. 231. Traité sur le venin de la vipere, &c. Florence, 1781, Tom. II. p. 169, 171.

[†] Ibid., p. 233 of the first of the above works, and 171 of the second.

1st. That the cause of all the motions of inspiration has its seat near that part of the medulla oblongata which gives rise to the nerves of the eighth pair.

2d. That the cause which animates each part of the body resides in the portion of the spinal marrow from which the nerves of that part are derived.

3d. That in like manner it is from the spinal marrow that the heart derives its life and its powers; but, from the whole spinal marrow, and not merely from any particular part of it.

4th. That the great sympathetic nerve takes its rise from the spinal marrow, and that the particular character of that nerve is to bring every part to which it is distributed under the immediate influence of the whole nervous power.

These results readily explain all the difficulties which have arisen since the days of Haller, respecting the causes of the motions of the heart. The reader will recollect that the principal of these are, 1st. Why does the heart receive nerves? 2d. Why is it influenced by the passions? 3d. Why is it not subjected to the will? 4th. Why does the circulation continue in acephalous and decapitated animals? He will recollect also, that till now no explanation has been able to reconcile these points, or at least has not been able to do so without the aid of hypotheses which we have seen give rise to other difficulties. But now we easily conceive why the heart receives nerves, and why it is so eminently subject to the influence of the passions, because it is animated by the whole of the spinal marrow. It does not obey the will, because none of the organs which are under the influence of the whole nervous power are subject to it. In fine, the circulation continues in acephalous and decapitated animals, because the motions of the heart do not depend on the brain, or only depend upon it in a secondary way. We ought to remark, that this last point, on which M. le Gallois has thrown so much light, presents only confusion and errors in authors of the old school of Haller, as well as in those of the new school. None of them have distinguished the motions of the heart which take place after decapitation, from those which we observe after the excision of this organ, or after the destruction of the spinal marrow; and they have thought that both were equally capable of maintaining the circulation. But these motions differ essentially. The latter have no power to support the circulation; they are quite similar to the feeble movements which we may excite in the other muscles for some time after death. M. le Gallois calls them motions of irritability, without attaching for the present any other meaning to the term, but that of expressing certain phenomena after death.

We have still one task to perform; to point out what particularly belongs to M. le Gallois in the work which is the object of this report, and what others are entitled to claim.

We can affirm, without fear of contradiction, that every thing in this work belongs to him. To be convinced of this, it is only necessary to read his memoir with attention. Chance suggested to him the idea of his first experiment, and that experiment led him to all the others, each of them being suggested to him, and as one may say, forced upon him by that which preceded it. In following him step by step, one observes that his own method has been his only guide, and that it is that alone which has inspired him. Thus, it is a thing without example in Physiology, to see a work of such length, in which all the parts are so connected, so dependent on each other, that to have the complete explanation of any one fact, it is necessary to recur to all those by which the author arrived at it, and in which it is impossible to deny one inference without denying all those which precede, and disturbing all those which follow it.

One might have expected that in researches so numerous, and which, by the importance of the questions they embrace, have commanded the attention of a great number of philosophers, the author would often have been led, even in confining himself to his own method, to repeat experiments which had been made by others; yet among all the experiments found in his memoir we have remarked only two which had been made before him; one by Fontana, the other by Stenon. The first* consists in inflating the lungs, and thus preserving the life of an animal after decapitation. Fontana only made that experiment to supply oxygen to the venous blood; and one may easily perceive that he was a stranger to the object before us. As the experiment was unconnected with any other subject, and did not serve as a proof of any point of doctrine, little attention was paid to it; and it was confounded with many other facts, shewing that even warm-blooded animals may live after decapitation without its being suspected that it was the decapitation which enabled them to live in that state. Hence it is that this experiment remained almost unknown except in some of the Schools of England and Germany; and M. le Gallois was wholly ignorant of it when he communicated to the Society of Medicine at Paris his first inquiries into the functions of the spinal marrow. Besides, this experiment in the hands of M. le Gallois was only one of the means by which he demonstrated two of his principal discoveries, namely, that the cause of the motions of inspiration has its seat in the medulla oblongata; and that the cause of life in the trunk resides in the spinal marrow.

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* Fontana. Traité sur le venin de la vipère, &c. Tom. I. p. 317.

The experiment of Stenon is that in which the ventral aorta is tied and then untied, to shew that the interruption of the circulation in any part occasions paralysis of that part, and that the return of the blood restores life to it. This experiment is well known, and has often been repeated. Some of the authors who have made it, had in view to prove that the contractions of the muscles depend on the action of the blood on their fibres; others that the sensibility of every part depends on the circulation; and in both views it served equally to prove or disprove the point, according to the manner in which it was made. Thus, when they secured the ventral aorta, the feeling and motion of the lower parts of the body quickly ceased *. But when the ligature was made lower, and only on one of the crural arteries, although in this case the circulation was wholly interrupted in the corresponding member, feeling and motion continued in it for a long time †. In these opposite results each author did not fail to adopt those which favoured his own opinion; and he thought himself authorized to do so, as the real cause of the difference was unknown.

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* Lorry, Journal de Méd. An. 1757, p. 15. Haller, Mém. sur le Mouvement du Sang, p. 203, Exp. 52.

† Schwenke, Hæmatol. p. 8. The 57th and 58th Experiments of Haller, p. 205, are of the same kind.

In the hands of M. le Gallois the same experiment shews itself under a very different aspect, and assumes a determined meaning. It is evident that feeling and motion ceasing in the hinder parts from a ligature being thrown round the aorta, arises from its being only in this case that the circulation is interrupted in that portion of the spinal marrow which gives rise to the nerves of these parts. Such are the only experiments of M. le Gallois, as far as we know, which can be claimed by others; but besides that the manner in which they make a part of his work renders them his own, it appears to us that the new points of view, under which he presents them, and the precision of the details and clearness of the results which he has substituted for the uncertainty and obscurity in which they were formerly involved, have made them experiments wholly new.

We shall finish by a few words on an opinion of M. Prochaska, which may be believed to be similar to that, which M. le Gallois has demonstrated respecting the functions of the spinal marrow. That author places the sensorium commune in the brain and spinal marrow conjointly*. But it is necessary to be aware that he thinks that the nervous power is generated throughout the whole extent of the nervous system, so that every part derives from its own nerves, taken alone, the cause of its life and of its movements*. He only regards the sensorium as a central point, where the nerves of feeling as well as those of motion meet and communicate, and which establishes the connexion between the different parts of the body †. On the contrary, M. le Gallois has demonstrated that the spinal marrow is not merely a means of communication between different parts, but that from it the cause of the life and power of the whole body proceeds. And what proves that M. Prochaska, in announcing his opinion, which besides he only mentions as a thing probable ‡, was far from suspecting the true functions of the spinal marrow, is, that he regards it as only a great bundle of nerves, *crassus funis nerveus* §.

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* Opera Minor. Tom. II. p. 51. Before him Marherr, Hartley, &c., had been of the same opinion.

In a word, it appears to us that we may say of the authors who have had some views on the subjects of which M. le Gallois treats, what M. Laplace has said with so much justice on a similar occasion. One may there meet with some truths, but they are almost always mixed with so many errors that their discovery belongs only to him, who, separating them from this mixture, succeeds by calculation or observation in effectually establishing them *.

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* Opera Minor. Tom. II. p. 82.

† Ib., p. 151.

‡ Ib., p. 153.

§ Ib., p. 48.

The opinion of your Committee is, that the work of M. le Gallois is one of the most excellent and certainly the most important which has appeared in Physiology since the learned experiments of Haller; that this work will make an epoch in that science over which it must spread a new light; that its author, so modest, so laborious, so meritorious, deserves that the class bestow on him its especial commendation, and all the encouragement which it can give. They cannot help adding, that the memoir of which they have given an account is worthy to occupy a distinguished place in the Transactions of learned correspondents, if the publicity of the important discoveries contained in it may be deferred to the time, perhaps distant, of the publication of those Transactions.

(Signed) DE HUMBOLDT.
HALLE'.
PERCY.

The Class approve their Report and adopt its conclusions.

It moreover decrees, that the Report shall be printed in the History of the Class, and that the

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* Mém. sur l'Adhésion des Corps à la Surface des Fluides dans la Biblioth. Britan. Tom. XXXIV. p. 33.

Committee of the Class shall make arrangements with M. le Gallois for defraying the expenses which have been occasioned by his experiments, and enabling him to continue them.

Certified to be conformable to the original.

G. CUVIER,
Perpetual Secretary.

CHAP. II.

Observations on the foregoing Report.

It will be necessary, before I enter on the account of my experiments, to make some observations on the foregoing report. As an account of the state of our knowledge of the subject at the time M. le Gallois began his experiments, it appears to be accurate, well arranged, and sufficiently comprehensive. As an account of the experiments and opinions of this author, nothing, as far as I can judge, can be more clear and correct; as an estimate of the merits of his work, it does not seem to me to deserve the same praise. It overlooks defects, both in his experiments and reasonings, of such moment as wholly to invalidate all his most important conclusions; and to leave him the discoverer of certain unconnected though most valuable facts, instead of the author of a new system, founded, as the report alleges, on a basis never to be shaken.

M. le Gallois has demonstrated, that the sudden destruction of any considerable portion of the spinal marrow so enfeebles the power of the heart, that it is no longer capable of supporting the circulation. He has also shewn that the same portion of the spinal marrow, whose sudden destruction destroys the circulation, may be destroyed by small parts without materially affecting it. The question then arises, if, as M. le Gallois supposes, the power of the heart is derived from the spinal marrow, and necessarily ceases when any considerable part of it can no longer perform its functions, why does the particular mode of destroying it make so great a difference in the result? This difficulty occasioned so much trouble to M. le Gallois, that it had nearly induced him to abandon the inquiry. "Après bien des efforts inutiles pour porter la "lumière dans cette ténébreuse question, je pris "le parti de l' abandonner, non sans regret d'y "avoir sacrifié un grand nombre d' animaux, "et perdu beaucoup de temps." Just before, he observes, "En un mot, j'eus presque au- "tant de résultats différens que d'expériences." And indeed the apparent contradictions in the results of M. le Gallois' experiments are such, as at first view to have persuaded me that some of his experiments were inaccurate; on repeating many of them, however, I was convinced of their accuracy. He attempts, we have seen, to explain the difficulty in the following manner. He has shewn that if ligatures be thrown round the large vessels, at no great distance from the heart, so as greatly to lessen the extent of the circulation, this organ can still support it, notwithstanding the destruction of such a portion of the spinal marrow as would, under ordinary circumstances, have destroyed it. On the same principle accoucheurs apply tourniquets to the limbs in cases of profuse uterine hemorrhagy. Now M. le Gallois supposes, that the power of the blood-vessels, as well as that of the heart, depending on the spinal marrow, we greatly impair the vigour of the circulation in any part by destroying that portion of the spinal marrow by which its nervous influence is supplied; and, therefore, that when any portion of the spinal marrow is destroyed by small parts, the vigour of the circulation in the corresponding parts of the body being greatly impaired, nearly the same effect is produced as if ligatures had been thrown round their vessels. It might here be objected, that when a considerable portion of the spinal marrow is at once destroyed, the power of the vessels corresponding to this portion being lost, the effect produced by the ligatures should still be observed. To this I suppose M. le Gallois would have replied, that as it requires some time for the destruction of any part of the spinal marrow to produce its effect on the vessels, when a large portion is destroyed at once, the vessels not accommodating themselves to the rapid destruction of the successive parts of the spinal marrow, the circulation is lost.

The foregoing explanation resting wholly on the position, that the vessels of any part are debilitated when deprived of the influence of the corresponding part of the spinal marrow, it was incumbent on the Committee to inquire by what experiments M. le Gallois had established it. This question, however, is overlooked by them; and, on reviewing the experiments of M. le Gallois, we find none from which any such inference can be drawn. He attempts to support it only by experiments not properly bearing on the point; although, if the position be correct, the simplest experiments are sufficient to establish it. It is impossible from his experiments to say, whether the diminished circulation in the parts in question arose directly from the destruction of part of the spinal marrow, or from the lessened power of the heart.

Another error of even greater consequence than the foregoing in the reasonings of M. le Gallois, which is also overlooked by the Committee, is his inference that the spinal marrow possesses an influence over the heart not possessed by the brain; because he found that removing the brain produces little or no effect on the action of the heart, while crushing the whole, or a considerable part of the spinal marrow, greatly enfeebles it. But to obtain this inference, it is evident that the brain and spinal marrow must be subjected to the same power. They ought both to have been removed or both crushed.

The inferences which M. le Gallois makes from the effects of crushing the spinal marrow are in another respect incorrect. There are two ways in which we may account for the power of the heart, or of the blood-vessels, being destroyed by crushing the spinal marrow. Either the heart and blood-vessels derive their power from the spinal marrow, and consequently lose it on the destruction of the whole or a considerable part of that organ; or, deriving their power from some other source, they are influenced by agents acting on the spinal marrow. It was incumbent on M. le Gallois, therefore, to ascertain by experiment in which of these ways crushing the spinal marrow produces the effects he observed. But he does not even seem aware, that it may act in any other way than that which he supposes.

By the same mode of reasoning, the inference which he draws from the restoration to life of the lower parts of an animal when a ligature, which has been thrown round the abdominal aorta, is removed, is inadmissible; namely, that when the circulation in every part is destroyed by crushing the spinal marrow, and we find that we cannot by any means restore it, this is to be ascribed to the absence of the influence of the spinal marrow. The same result may arise, it is evident, from the heart and blood-vessels, supposing them to derive their power from some other source, being so deranged by a powerful agent acting through the spinal marrow, that they are no longer capable of performing their functions. M. le Gallois relates no experiment to prove that his explanation ought to be admitted in preference to this; and the Committee speak as if no inference, but that of M. le Gallois, could be drawn from the experiment.

Nor is M. le Gallois' inference respecting the origin of the great sympathetic nerve warranted by his experiments; namely, that it arises wholly from the spinal marrow. It is true that he has found, that through this nerve a powerful agent, applied to any considerable portion of the spinal marrow, is capable of enfeebling the power of the heart; but nothing said by M. le

Gallois proves that the heart may not also be affected in the same way through the brain.

A position on which much of the reasonings of M. le Gallois rests, which is admitted by the Committee, but of which we find no proof in the experiments of this author, is, that the contractions of the heart, after the spinal marrow is crushed, are of a nature different from those which support the circulation. Observing that after the spinal marrow is thus destroyed, the contractions of the heart are too feeble to support the circulation, without farther inquiry he concludes, that these contractions do not merely differ in degree from those which support the circulation, but, existing independently of the spinal marrow, are wholly of a different nature. The contractions of the heart, after it is removed from the body, are regarded by M. le Gallois as analogous to those which remain after the spinal marrow is crushed, and he regards in the same light the contractions which may be excited for a short time after death in the muscles of voluntary motion. Had M. le Gallois' mind been unbiassed by his peculiar views of the subject, he would have easily observed a striking difference between the action of the heart, immediately after the spinal marrow is crushed, and its action immediately after it is removed from the body. In the former instance it is feeble and fluttering, gradually, especially in the cold-blooded animal, becoming stronger and more regular; in the latter instance, it is comparatively strong and regular, gradually, and in the cold-blooded animal very slowly, becoming more feeble. With respect to the contractions of the muscles of voluntary motion after death, it is generally known that these muscles may for some time be excited to the perfect performance of their function. They can be made to move the limbs precisely as they did before the death of the animal. But whether they move them as forcibly or not, and whether or not the heart beats as forcibly after it is removed from the body, as while it supported the circulation, as far as we can see, the action of both is of the same nature as when they performed their usual functions; and M. le Gallois has adduced no proof whatever of its being of a different nature. The experiment, indeed, in which he lessens the extent of the circulation by ligatures, and thus enables the heart to support it after such a portion of the spinal marrow is crushed, as would otherwise have destroyed it, is a sufficient refutation of his own opinion. It proves that the effect of crushing a large portion of the spinal marrow is merely that of enfeebling, not changing the nature of the action of the heart.

Another position of M. le Gallois, admitted by the Committee, which does not seem to be warranted by his experiments, is, that the power, on which all the motions of inspiration depend, has its seat near that part of the medulla oblongata, which gives rise to the eighth pair of nerves. On this subject I shall hereafter have occasion to make many observations; and shall only remark here, that inspiration is a complicated function; and that if any of the powers necessary to it are withdrawn, its most essential motions are as quickly destroyed as if all these powers had ceased. Now M. le Gallois made no experiments to ascertain whether it is by the destruction of one or all of these powers, that inspiration is destroyed by destroying this part of the medulla oblongata.

The argument employed by the Committee in favour of M. le Gallois' opinions from the existence of acephalous fœtuses, is wholly invalidated by the fact, that fœtuses have been born alive without either brain or spinal marrow; for instances of which M. le Gallois himself refers, in the two hundred and fifty-first page of his Treatise, to the *Hist de l'Acad. des Sciences, An.* 1711, *Obs. Anat.* 3, and *An.* 1712, *Obs. Anat.* 6, but without attempting to shew how it is possible to reconcile his opinions with the existence of such cases. The reader will find, in the fifth volume of the Medico-Chirurgical Transactions, the case of a full-grown fœtus, without either brain or spinal marrow, described by Mr. Lawrence. I have seen a similar case. In this case, as in that mentioned by Mr. Lawrence, in the place of the spinal marrow there only appeared a vascular membrane.

An inconsistency, of great importance in M. le Gallois' work, which he makes no attempt to explain, is overlooked by the Committee. He observes, in the commencement of his work, "Ce que j'y ai dit du cœur pouvant s'appliquer "aux autres organes des fonctions involontaires, la question peut étre considérée plus "généralement, comme la determination du "siége du principe qui préside à cet ordre de "fonctions*." Yet he shews that decapitation does not influence the function of the heart, while the division of the eighth pair of nerves injures that both of the lungs† and stomach.

It appears from what has been said, as far as I am capable of judging, that the experiments of M. le Gallois do not warrant any of the following positions stated by the Committee as the result of these experiments.

"1°. Que le principe de tous les mouvemens "inspiratoires a son siége vers cet endroit de la "moëlle allongée qui donne naissance aux nerfs "de la huitième paire;

"2°. Que le principe qui anime chaque partie

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* Avant-propos, page 1.

† I speak here of the function of the lungs themselves, not of the muscles of inspiration.

" du corps réside dans ce lieu de la moëlle épi-
" nière duquel naissent les nerfs de cette partie;

" 3º Que c'est pareillement dans la moëlle
" épinière que le cœur puise le principe de sa
" vie et de ses forces ; mais dans cette moëlle
" toute entière, et non pas seulement dans une
" portion circonscrite ;

" 4º Que le grand sympathique prend nais-
" sance dans la moëlle épinière, et que le carac-
" tère particulier de ce nerf est de mettre cha-
" cune des parties, auxquelles il se distribue
" sous l'influence immédiate de toute la puis-
" sance nerveuse ;" that is of the whole of the spinal marrow, which M. le Gallois regards as the seat of the nervous influence.

If these results are not legitimate inferences from the experiments of M. le Gallois, the explanations of the long-contested points respecting the action of the heart, founded on them, are inadmissible ; namely, That the heart is supplied with nerves because it derives its power from the spinal marrow ; That it is influenced by the passions, because the brain acts upon it through the spinal marrow ; That it does not obey the will, because no organ influenced by every part of the nervous power, that is, of the spinal marrow, does obey the will ; (it may here be remarked, that were this position admitted, it would by no means explain why the motions of the heart are independent of the will, though influenced by the passions); and, That the circulation continues in acephalous and decapitated animals, because its direct dependence is not on the brain, but on the spinal marrow.

If the foregoing observations are correct, we must dissent from the following opinion of the Committee: " Ces résultats resolvent sans peine " toutes les difficultés qui se sont élevées depuis " Haller sur les causes des mouvemens du " cœur." The experiments of M. le Gallois, indeed, by ascertaining some facts of great importance, while others immediately connected with them escaped his observation, have left the subject in greater confusion than he found it. Instead of removing the difficulties which formerly existed, the valuable additions which he has made to our knowledge have shewn us others.

The heart's being subject to the passions, yet independent of the influence of the brain, on which so much has been written, does not seem to imply a more direct contradiction, than that the destruction of the same part of the spinal marrow should, according to the way in which it is effected, either destroy the function of the heart, or little, if at all, influence it. I have had occasion to observe, that M. le Gallois' explanation of this apparent contradiction is not a legitimate inference from his experiments; and I shall soon relate some, so simple that it is impossible to be deceived in their result, which seem directly to refute that explanation.

Why, if the power of the heart depends on the spinal marrow, as it appears to do from the experiments of M. le Gallois, the accuracy of which I have ascertained by repeated trials, have fœtuses been born alive where no spinal marrow had ever existed?

Why, if the power of the heart depends on the spinal marrow, does it continue to perform its usual motions after it is removed from the body?

Why, if (as M. le Gallois maintains, and it is generally admitted,) the various organs of involuntary motion bear the same relation to the nervous system, is the function of the heart uninfluenced by decapitation, and that of the stomach immediately impaired by dividing or throwing a ligature round the eighth pair of nerves?

Why does respiration cease on the destruction of a certain part of the medulla oblongata, since the nerves of the diaphragm and intercostal muscles arise from the spinal marrow, which M. le Gallois has proved to be capable of exciting the muscles independently of the brain? He considers this subject at length in the thirty-fifth and following pages of his treatise, and admits that he can give no explanation of it, calling it " one of the great mysteries of the nervous power, the discovery of which will throw the strongest light on the mechanism of the functions of that wonderful power."

These apparent contradictions, it is evident, as well as those which existed before the discoveries of M. le Gallois, must be reconciled, before we can be said to understand the relation which the thoracic and abdominal viscera bear to the nervous system. The doctrine which cannot reconcile them must be erroneous.

PART II.

Experiments made with a View to ascertain the Laws of the Vital Functions.

THE sanguiferous system, it is evident, may be divided into three parts, whose functions differ; the heart, the vessels of circulation, and the vessels of secretion. In the following Inquiry, I shall, in the first place, endeavour to ascertain the principle on which the action of the heart and the vessels of circulation depends, and the relation which subsists between them and the nervous system. I shall then consider the principle on which the action of the muscles of voluntary motion depends, and the relation which they bear to this system. The comparative effects of stimuli, applied to the brain and spinal marrow, on the heart and muscles of voluntary motion, will next be considered. An account of the experiments on these branches of the subject, though not in the order in which they are here related*, was presented to the Royal Society in two papers, composed while I was still engaged in the Inquiry, and published in the Philosophical Transactions of 1815.

The next object of inquiry will be the principle on which the action of the secreting vessels depends, and the relation which they bear to the nervous system. I shall then endeavour to ascertain the principle on which the action of the alimentary canal depends, and the relation which it bears to this system. These subjects will lead to some experiments and observations on the use of the ganglions, the cause of animal temperature, and the nature of the nervous influence. I shall then consider the relation which the different functions of the animal body bear to each other, and the order in which they cease in dying; and the Inquiry will conclude with a review of the inferences obtained from the various experiments and observations which will be laid before the reader, and some remarks on their application to explain the nature and improve the treatment of diseases.

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* The experiments which I have since made have led to conclusions which render a different arrangement necessary.

CHAP. I.

On the Principle on which the Action of the Heart and Vessels of Circulation depends.

As it is now generally admitted by Physiologists, as appears from the report just laid before the reader, that the heart is capable of performing its functions after the brain is removed, the first question which presents itself is, How far does the power of this organ depend on the influence of the spinal marrow, from which, we have seen, M. le Gallois maintains, that it is wholly derived?

Exp. 1. A rabbit was deprived of sensation and voluntary power by a blow on the occiput. When the rabbit is killed in this way, the respiration immediately ceases; but the action of the heart and the circulation continue, and may be supported for a considerable length of time by artificial respiration, as practised by Fontana, and since by Chirac, Mr. Brodie, M. le Gallois, and others*. This mode of destroying the sensibility does not influence the result of the experiment, and has the double advantage of preventing the animal's sufferings, and his motions. Its greatest inconvenience is, that if the blow is very severe, considerable vessels are sometimes ruptured, and there is almost always some rupture of vessels, which of course tends to impair the vigour of the circulation.

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* It appears from the first volume of the Philosophical Transactions, that Mr. Hook, in the year 1667, shewed in the presence of the Members of the Royal Society, not

The spinal marrow was laid bare from the occiput to the beginning of the dorsal vertebræ, the circulation being supported by artificial respiration. The chest was then opened, and the heart found beating regularly, and with considerable force. The spinal marrow, as far as it had been laid bare, was now wholly removed, but without in the least affecting the action of the heart. After this, the artificial respiration being frequently discontinued, we repeatedly saw the action of the heart become languid, and increase on renewing it. The skull was then opened, and the

only that the life of a dog could be preserved for an hour after the thorax had been opened, and a great part of the diaphragm removed, by alternately inflating the lungs and allowing them to collapse so as to imitate respiration; but that the effect is nearly the same if the lungs are preserved in a state of permanent distension, by air constantly thrown into them, and allowed to escape by small perforations made in their surface.

whole of the brain removed, but without any abatement of the action of the heart, which still continued to be more or less powerful, according as we discontinued or renewed artificial respiration. This being for a considerable time discontinued, the ventricles ceased to beat about half an hour after the removal of the brain. On renewing the respiration, however, the action of the ventricles was restored. The respiration was again discontinued and renewed, with the same effects.

Exp. 2. A rabbit was made insensible by removing part of the skull, and applying opium to the brain. The spine was then opened between the cervical and dorsal vertebræ. We then laid open the thorax, and supported the action of the heart by artificial respiration. The force with which it beat was carefully observed, and the spinal marrow destroyed by running a small hot wire up and down the spine, through the opening made in it, by which the action of the heart was not at all affected.

Exp. 3. In the foregoing experiments, it may be said, there was no direct proof of the continuance of the circulation after the spinal marrow was destroyed or removed. On this account several of the following experiments were made. A rabbit was deprived of sensation by a blow on the occiput, and the circulation supported by artificial breathing. The carotids being exposed were seen beating. The cervical part of the spinal marrow was then destroyed by a hot wire, after which the carotids were still found perfectly round and beating.

Exp. 4. In a rabbit rendered insensible by a blow on the occiput, the whole spinal marrow, was destroyed by a hot wire, and the breathing artificially supported. One of the carotid arteries was then laid bare. Its beating was evident, and on dividing it, florid blood flowed from it freely.

Exp. 5. The only difference between this and the last experiment was, that artificial breathing was not performed. In both, the spinal marrow was destroyed, by introducing a wire hot enough to make a hissing noise through an opening between the cervical and dorsal vertebræ, first through the upper portion into the brain, then through the under portion to the end of the spine. On laying open one side of the neck, the carotid artery was found beating. On dividing it, blood of a much darker colour than in the former experiment was thrown out copiously *per saltum*.

Exp. 6. A rabbit was rendered insensible by a blow on the occiput, and artificial respiration maintained. The spinal marrow from the base of the skull to the beginning of the dorsal vertebræ was removed, and the remaining part of it destroyed by a hot wire. The carotid artery was then found beating, and, on dividing it, florid blood rushed out with great force *per saltum*.

Exp. 7. This experiment resembled the last, except that the spinal marrow, instead of being partly removed, was wholly destroyed by a hot wire, and artificial breathing was not performed previous to opening the carotid, from which dark coloured blood flowed *per saltum*. We then inflated the lungs, and florid blood soon began to flow copiously from the vessel, and appeared like a red stream mixing with the dark coloured blood which had previously come from it. This experiment was repeated in the same manner, and with the same result.

Exp. 8. In this experiment the rabbit was rendered insensible, by the blow on the occiput, except that the breathing still continued. The spine was opened, and the spinal marrow destroyed, as in the preceding experiment. A small wire was used very hot. On introducing it through the spine into the brain, the breathing immediately ceased. The femoral artery was laid bare about two or three minutes after respiration had ceased. The beating of the artery was evident. On opening it, a dark coloured blood flowed from it freely. We now had recourse to artificial respiration. When it had been employed for about half a minute, the blood, which continued to flow copiously from the artery, became of a highly florid colour. The other femoral artery was then opened, from which florid blood also flowed freely. When about an ounce of blood had flowed from the two vessels, the inflation of the lungs was discontinued, and the blood again flowed of a dark colour. On renewing the inflation of the lungs, in less than half a minute, it became of a florid colour. It continued to flow from the femoral arteries altogether for seven minutes. Three minutes after the blood had ceased to flow from them, artificial respiration being continued, one of the carotid arteries was opened, from which a florid blood flowed in a free stream, to the amount of a drachm and a half. The flow from the carotid artery ceased in eleven minutes after the femoral artery had been opened. Most of the blood was now of course evacuated. A good deal had been lost in opening the spine, which always happens. The left auricle and ventricle were found nearly empty. The blood which remained in them was florid. The right auricle and ventricle were full of dark blood.

Exp. 9. From various trials, we found that in such experiments the circulation ceases quite as soon without, as with the destruction of the spinal marrow. Loss of blood seems to be the chief cause which destroys it. If the animal were operated upon without being rendered insensible, pain would also contribute to this effect. We frequently, after laying open the skull and spine, found the circulation lost before either the brain or spinal marrow had been disturbed. The circulation is particularly apt to fail, if artificial respiration is not carefully performed after the animal ceases to breathe. In making such experiments, after opening the bone, it is always necessary to ascertain whether the circulation continues, before we destroy or remove the brain or spinal marrow. As little blood is lost in this part of the operation, when the carotid arteries are beating before, we always find them beating after it. The result of this experiment is still more striking in cold-blooded animals, in which death takes place so slowly, that the circulation continues long after the destruction of the brain and spinal marrow.

Exp. 10. The brain of a frog and the spinal marrow as low as the dorsal vertebræ were laid bare. The thorax was then opened, and the heart found acting vigorously; and from the transparency of its sides, the passage of the blood through it distinctly seen. The part of the spinal marrow which had been laid bare was then removed, but without at all affecting either the motion of the heart, or the passage of the blood through it. The brain was then removed, with the same result.

Exp. 11. The brain and spinal marrow of a frog were removed at the same time. On opening the thorax, the heart was found performing the circulation freely.

It appears from these experiments that the action of the heart is as independent of every part of the spinal marrow as of the brain; and, consequently, that the opinion of M. le Gallois that it derives its power from that organ, and particularly from the cervical part of it, must be regarded as erroneous. I shall soon have occasion to consider the facts which led M. le Gallois to this opinion; we shall find, I think, that they admit of a very different explanation. We are now to inquire, whether the action of the vessels of circulation is also independent of the brain and spinal marrow.

The following experiments, and some others which I shall have occasion to relate, were made on the capillaries of the frog, which, from the extent and transparency of the web of its hind feet, and from its great tenacity of life, appeared the best subject for such experiments. It has been questioned, how far inferences, drawn from experiments made on cold-blooded animals, can be supposed to apply to those of warm blood. Both Fontana and Dr. Monro observe, that in their experiments they found the system of both obeying the same laws. The experiments I have had occasion to make on both sets of animals tend to confirm this observation. There are certain circumstances in which they evidently differ, in all others they seem to agree. As there is no part of the warm-blooded animal on which such experiments on the vessels of circulation, as I shall have occasion to relate here and in the next chapter, can be made except the mesentery, many of them would be attended with much greater suffering in this, than in the cold-blooded animal. Some of them, from the warm-blooded animal being less tenacious of life, could not be so satisfactorily performed on it.

Exp. 12. A strong ligature was thrown round the neck of a frog, and the head cut off without any loss of blood; much loss of blood immediately destroys the circulation in the extremities. The spinal marrow was then destroyed by a small wire. On bringing the web of one of the hind legs before the microscope, I found the circulation in it vigorous for many minutes, and in all respects resembling that in the web of a healthy frog. This experiment was repeated with the same result.

Exp. 13. The spinal marrow of a frog was destroyed by moving, in various directions, a wire introduced into the spine by a hole made in the lowest part of it, and passed up into the brain. The animal was immediately deprived of sensibility and voluntary motion, and appeared to be quite dead. After it had lain in this state for several minutes, part of the web of one of the hind legs being brought before the microscope, the blood was seen circulating in it as rapidly as in the web of a healthy frog. In making such experiments it is necessary to be aware, that handling and stretching the web tends to impair the vigour of the circulation in it. If this experiment is objected to on account of its being made on an animal of cold blood, I may, as far as the larger vessels, are concerned, refer to several experiments just related, in which the carotid and femoral arteries of rabbits were found beating and supporting the circulation after the sensibility had been destroyed by a blow on the head, and the spinal marrow had been removed or destroyed.

It appears from these experiments that the vessels of circulation, like the heart, retain their power after the brain and spinal marrow are destroyed or removed, for it will hardly be maintained, that in these instances the power of the heart supports the motion of the blood in the vessels. Should this opinion be maintained, the reader will find it refuted, respecting animals of cold blood, by experiments related in the next chapter, and respecting animals of warm blood, by those related in chapter the tenth.

From the whole of the foregoing experiments we must infer, that the position by which M. le Gallois explains why the destruction of the same portion of the spinal marrow destroys the circulation if suddenly effected, but fails to do so if effected slowly, is erroneous *.

Does it not seem a necessary inference, from the experiments related in this chapter, that the action of the heart and vessels of circulation depends on a power inherent in themselves, and having no direct dependence on the nervous system ? Yet many facts, laid before the reader in the first part of this Inquiry, prove that a certain relation subsists between the nervous and sanguiferous systems. What this relation is we are now to inquire.

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* See page 54, et seq.

CHAP. II.

On the Relation which subsists between the Heart and Vessels of Circulation and the Nervous System.

It is generally admitted, we have seen, that the action of the heart cannot be influenced by stimuli applied to the brain and spinal marrow: and it seems almost a contradiction to suppose that it should be so, when we see that it cannot be influenced by the total removal of these organs. There were many reasons, however, which induced me to try the effect on the heart of stimuli so applied to the brain and spinal marrow, as not to excite any of the muscles of voluntary motion, whose action, both by throwing more blood towards the heart, and by agitating the animal, prevents our judging of the effect of the stimulus.

Exp. 14. A rabbit was deprived of sensation and voluntary motion by a blow on the occiput, the action of the heart supported by artificial respiration, and the brain and cervical part of the spinal marrow laid bare. The thorax was now opened, and the action of the heart, which beat with strength and regularity, observed. Spirit of wine was then applied to the spinal marrow, and a greatly increased action of the heart was the consequence. It was afterwards applied to the brain with the same effect. The increase of motion was immediate and decided in both cases. We could not perceive that it was greater in the one case than the other.

Exp. 15. The foregoing experiment was repeated, with the difference, that the whole of the spinal marrow was laid bare. The motion of the heart was nearly, if not quite, as much influenced by the application of the stimulus to the dorsal, as to the cervical portion of the spinal marrow; but it was very little influenced by its application to the lumbar portion.

Exp. 16. In this experiment, only that part of the brain which occupies the anterior part of the head was laid bare. The rabbit in other respects was prepared in the same way as in the preceding experiments. The spirit of wine applied to this part of the brain, produced as decided an effect on the motion of the heart as in those experiments. The spirit of wine was washed off, and a watery solution, first of opium, then of tobacco, applied, with the effect of an increase, but a much less increase of the heart's action than arose from the spirit of wine. The increased action was greater from the opium than from the tobacco.

The first effect of both was soon succeeded by a more languid action of the heart than that which preceded their application to the brain. This effect was greatest, and came on soonest when the tobacco was used; and we always, for we frequently repeated the experiment, saw an evident increase in the action of the heart when we washed off the tobacco. We could also perceive this, though in a less degree, when the opium was washed off. Little or none of this debilitating effect was observed when the spirit of wine was used. After its stimulating effect had subsided, the action of the heart only returned to about the same degree as before the application of the stimulus.

Exp. 17. The foregoing experiment was repeated on an animal of cold blood. Mr. Hastings had found, that immersing the hind legs of a frog in tincture of opium, in less than a minute deprives it of sensibility. This does not arise from any action of the opium; a watery solution of opium, we found, however strong, does not produce the effect. It is immediately produced by simple spirit of wine, and arises from the action of the spirit on the nerves of the part to which it is applied, for it takes place quite as readily as in the healthy frog, after a ligature has been thrown round the vessels attached to the heart. It is remarkable, that if simple spirit of wine is used, the animal expresses severe pain, if tincture of opium, very little. I have just mentioned the reason why it is necessary, in order to judge of the result of this experiment, that the animal should be rendered insensible.

Having thus deprived a frog of sensibility, we laid bare the brain and spinal marrow, and opened the chest. The heart was found contracting with vigour. Spirit of wine was applied to the spinal marrow, with an immediate and evident increase of the action of the heart. It was then applied to the brain with the same effect. Watery solutions of opium and tobacco were also applied to both, with precisely the same effect as in the rabbit. The increase of action from the opium and tobacco was much less than from the spirit of wine, and was soon followed by a great diminution of action. The increase of action was least, and the diminution greatest from tobacco. On washing off the opium and tobacco with a wet sponge, the heart immediately beat more strongly. The different parts of this experiment were frequently repeated with the same result. It is remarkable that we could affect the motion of the heart by stimuli applied to the brain and spinal marrow, after they had all ceased to produce any effect on the muscles of voluntary motion through the medium of the nervous system.

Exp. 18. This experiment only differed from the last in the cervical part of the spinal marrow and lower part of the brain being removed, and the stimuli applied only to that part of the brain which lies between the eyes of the frog. Spirit of wine, opium and tobacco, thus applied, affected the motion of the heart quite as much, and precisely in the same way, as when they were applied to the entire brain or spinal marrow. When opium and tobacco were applied to the lower part of the spinal marrow, the motion of the heart appeared to be hardly at all affected by them. It was evidently increased when spirit of wine was applied to the same part.

We found in the foregoing experiments, that considerable pressure either of the brain or spinal marrow produces little or no effect on the action of the heart. Its action could be influenced by agents applied to the brain and spinal marrow long after the circulation had ceased.

Thus we see that the heart, although its power is independent of the brain and spinal marrow, is capable of being influenced through these organs.

All that has been said of the power of

the heart is strikingly illustrated by the following experiments.

Exp. 19. If the head and spinal marrow of a frog be removed, we have seen, the heart continues to perform its function perfectly for many hours, nor does it seem at all immediately affected by their removal. But we find the effect very different when the most sudden and powerful agent is applied to them. If they are even destroyed by being cut to pieces, the heart after their destruction beats just as before it. But if either the brain or spinal marrow be instantly crushed, the heart immediately feels it. The thorax of a large frog was laid open, and the motion of the heart observed, which performed the circulation perfectly and with great force. The brain was then crushed by the blow of a hammer. The heart immediately performed a few quick and weak contractions. It then lay quite still for about half a minute. After this its beating returned, but it supported the circulation very imperfectly. In ten minutes its vigour was so far restored that it again performed the circulation with freedom, but with less force than before the destruction of the brain. An instrument was then introduced under the heart, and after ascertaining that this had produced no change on its action, the spinal marrow was crushed by one blow, as the brain had been. The heart again beat quickly and feebly for a few seconds, and then seemed wholly to have lost its power. In about half a minute it again began to beat, and in a few minutes acquired considerable power, and again supported the circulation. It beat more feebly, however, than before the spinal marrow was destroyed. It ceased to beat in about an hour and a half after the brain had been destroyed. In another frog, after the brain and spinal marrow had been wholly removed, the heart beat nine hours, gradually becoming more languid.

In this experiment we see that the heart not only retains its power long after the brain and spinal marrow are removed, but that if they are destroyed in such a way as to impair and almost destroy the action of the heart, it can recover the power of performing its function, after they no longer exist; precisely as a muscle of voluntary motion will by rest recover its excitability, although all its nerves are divided.

Exp. 20. The foregoing experiment cannot be performed in the same way on warm-blooded animals, but it may be performed in a way equally satisfactory. In two rabbits the brain was crushed by a blow. In both the heart immediately beat with an extremely feeble and fluttering motion. The anterior part of the brain only was crushed in another rabbit, with the same result. A strong ligature was thrown round the neck of a fourth rabbit, and at the moment it was tightened, the head was cut off. The bleeding was restrained by the ligature, except from the vessels defended by the bone. General spasms made the body hard for the space of between one and two minutes, so that the beating of the heart could not be felt. At the end of this time, the heart was felt through the side, both by Mr. Hastings and myself, beating regularly, and not more quickly than in health. All the rabbits used in this experiment were of the same age.

Exp. 21. The following experiment is still more conclusive. The anterior part of the brain of a rabbit was crushed by a blow. The side was rendered hard by spasm for about half a minute. Neither during this, nor after it, could I perceive any motion of the heart by applying the hand to the side. The head was then cut off, about three quarters of a minute after the brain had been crushed. No blood spouted out, and very little ran from the vessels. A strong ligature was passed round the neck of another rabbit of the same age. It was suddenly tightened, and the head cut off. In this instance little spasm took place, and the heart was found beating regularly under the finger for about three quarters of a minute. At the end of this time the ligature was slackened, and the blood spouted out to the distance of three feet, and continued to spout out with great force, till nearly the whole blood was evacuated.

Exp. 22. From the strength of the spine of a rabbit, and the situation of the neighbouring parts, it is impossible to crush it, without directly influencing the state of the heart by the blow. We opened it between the cervical and dorsal vertebræ, and suddenly forced a steel rod, of considerable thickness, through the cervical part. As in the experiments of M. le Gallois, the action of the heart was immediately debilitated. In the preceding experiments, the reader has seen, we repeatedly slowly destroyed, or removed entirely, both the cervical and other portions of the spinal marrow, without at all influencing the action of the heart.

These experiments point out an easy solution of the difficulties mentioned by M. le Gallois in the 119th and following pages of his treatise. When the greater part of the spinal marrow was destroyed by small portions at a time, comparatively little effect was produced on the heart; but when a considerable part of it was crushed at once, the power of the heart was so impaired, that the circulation ceased. Thus in other cases, where the injury was inflicted slowly, and where it was inflicted suddenly, the result was found to be different. He observes, that if the spinal marrow be divided near the occiput, and a certain part of it immediately destroyed, the circulation ceases. If some time intervene between the division and the destruction of precisely the same part, the circulation is not interrupted.

In M. le Gallois' experiments, the spinal marrow was always crushed by a stilet, of precisely the same dimensions with the cavity of the spine. In the experiments above related, the spinal marrow was either removed, or destroyed by a comparatively small wire, moved about in it till its functions ceased. The reader will easily understand, from what has been said, why this apparently slight circumstance occasions so essential a difference in the result of the experiments. We have just seen the difference of the result when the brain or any portion of the spinal marrow is crushed at once, and when they are destroyed by parts, or wholly removed.

M. le Gallois maintains, we have seen, that affections of the brain influence the heart only through the medium of the spinal marrow. But in experiment 18th, we have seen, that after the lower part of the brain and the spinal marrow of a frog had been removed, agents applied only to that part of the brain which lies between the eyes, affected the action of the heart as much as when applied to the brain, while both this organ and the spinal marrow were entire. To remove any objection which may arise from the subject of this experiment having been an animal of cold blood, the following was made :—

Exp. 23. I divided the spine of a rabbit near the head, (which by stopping the respiration deprived the animal of sensibility and voluntary power), and then at the lower end; and by means of a wire, introduced at these parts, destroyed the spinal marrow. Spirit of wine was then applied to the brain, which influenced the action of the heart as readily, and to as great a degree, as it does when the spinal marrow is entire.

We are now to inquire how far the vessels of circulation are capable of being influenced through the brain and spinal marrow.

In order to ascertain whether the vessels can be stimulated through these organs independently of their action on the heart, it is necessary, in the first place, to determine how far the vessels can support the motion of the blood independently of the heart.

M. Bichat *(Recherches Phys. sur la vie et la mort)* has shewn that in a frog the motion of the blood continues in the capillaries after the heart no longer propels it. This observation indeed, we shall afterwards find applies to the warm, as well as the cold-blooded animal.

Exp. 24. A ligature was thrown round the vessels attached to the heart of a frog, and the heart was then cut out. On bringing the web of one of the hind legs before the microscope, the circulation in it was found to be vigorous, and continued so for many minutes; at length gradually becoming more languid.

In endeavouring to proceed farther, I found much difficulty. It was not only necessary, in order to ascertain the effect of stimuli applied to the brain or spinal marrow on the vessels of the web, to remove the heart, and to lay open the cranium or spine, but also to prevent the voluntary motions of the animal, which continually occur, and never fail to accelerate the motion of the blood in the web.

Exp. 25. A frog was deprived of sensibility and voluntary motion, by the upper parts of the body being immersed in laudanum; part of the cranium was then removed, after a ligature had been thrown round the neck to prevent loss of blood. The thorax was now opened, and the vessels attached to the heart included in a ligature. But, notwithstanding this experiment was repeatedly performed with the greatest care, the circulation by all these preparatory means was so enfeebled, that although the blood still moved in the web, it was in so irregular and uncertain a way, that I never could arrive at any positive conclusion respecting the effect of the stimulus applied to the brain. After many fruitless attempts, therefore, I abandoned this mode of making the experiment.

Although the action both of the heart and the muscles of voluntary motion so influence the effect of stimuli applied to the brain, on the circulation in the foot, that, without wholly preventing the effect of both, no conclusion can be drawn, it is evident that the action of the latter cannot increase the effect of sedatives; and the sedative lessening the power of the heart will not affect the result of the experiment, if it be made on the web of the frog. We have just seen, that the total ceasing of the action of the heart does not, for a considerable time, affect the circulation in it. The following experiments appear to be decisive of the effect of the sedative, and of the stimulus, as far as this can be decisive, the action of the heart remaining. It is evident that the action of either stimulus or sedative is equally conclusive respecting the direct influence of the nervous system on the blood vessels.

Exp. 26. Part of the cranium of a frog was removed, the web of one of the hind legs brought before the microscope, and the circulation in it observed. The animal was now rendered insensible by the immersion of the other hind leg in laudanum. The insensibility did not in the least affect the circulation in the web before the microscope. Spirit of wine was then applied to the brain with an evident increase of the velocity of the blood in the web. The same effect was produced in a less degree by watery solutions of opium and tobacco. After the tobacco had been applied for about half a minute, the motion of the blood was much less rapid than before its application. On washing off the tobacco the velocity of the blood increased, and was again lessened on applying it. This was repeated several times with the same effects. The following way of performing the experiment is equally conclusive.

Exp. 27. A frog was rendered nearly insensible by having its back immersed in laudanum. A ligature was then thrown round the neck to prevent loss of blood, part of the cranium removed, the web of one of the hind legs brought before the microscope, and the circulation in it, which was rapid, observed. A strong infusion of tobacco was then applied to the brain, with the effect of at first rendering the circulation more rapid. In about half a minute it became more languid, and soon stopped altogether. On the infusion of tobacco being washed off, the circulation returned and regained considerable vigour. The tobacco was several times applied to the brain and washed off, with the same effects. I may observe, that when the circulation in the web had almost ceased after the tobacco had been washed off, its velocity was immediately increased on applying spirit of wine to the brain.

The power of the blood-vessels, like that of the heart, is capable of being directly destroyed through the medium of the nervous system.

Exp. 28. The web of one of the hind legs of a frog was brought before the microscope, and while Mr. Hastings observed the circulation, which was vigorous, I crushed the brain by the blow of a hammer. The vessels of the web instantly lost their power, the circulation ceasing ; an effect which cannot arise, we have seen, from the ceasing of the action of the heart. In a short time the blood again began to move, but with less force. This experiment was repeated with the same result. If the brain is not completely crushed, the blow increases the rapidity of the circulation in the web.

Exp. 29. The spine of a frog was laid open at the lower end, and a wire of nearly the same dimensions with its cavity, forced through it, as in M. le Gallois' experiments. The web of one of the hind legs was then brought before the microscope, and the circulation in it was found to have wholly ceased. In another frog, as we have seen *, the spinal marrow was destroyed by introducing in the same way, and moving in various directions, a wire much smaller than the cavity of the spine. The frog soon appeared to be quite dead, but the circulation in the web was found to be vigorous.

Exp. 30. Analogous to what happened with respect to the heart, as I shall have occasion to point out more particularly in the fourth chapter, I could never, either by chemical or mechanical agents, if we except crushing the brain or spinal marrow, excite any irregular action in the blood-vessels. Their action was only rendered more or less powerful.

The irregular appearances in the circulation in the web of a frog's foot, mentioned by Dr. Thompson, Professor of Military Surgery in the University of Edinburgh, in his Lectures on Inflammation lately published, and which he ascribes to inflammation, may be observed in any case, if the vessels be at all compressed in applying the foot to the microscope; and although they are not compressed, these appearances very generally occur when the circulation begins to fail. The blood will then stop and go on at intervals, and move backwards and forwards in the same vessel. I have often watched the capillaries from the commencement of inflammation to its greatest height, when the part is about wholly to lose its vital power, in the mesentery of a rabbit, the web of a frog's foot, and the fins of fishes, without perceiving the least tendency to this irregular motion when the part viewed was so applied to the microscope as not to compress any of its vessels*. When the circulation fails without any morbid distension of the vessels, the motion of the blood becomes irregular before it stops altogether; when it fails from morbid distension of the vessels, which gives rise to the phenomena of inflammation, this irregularity is not perceived, the motion of the blood gradually becomes slower till it ceases altogether.

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* See Experiment 13.

What are the simple results of the experiments related in this and the preceding chapter? The first set prove, that the power of the heart and vessels of circulation is independent of the brain and spinal marrow, for we find that the functions of the former organs continue after the latter are destroyed or removed, and that their removal is not attended with any immediate effect on the motions of the heart and vessels. The second set prove, that the action of the heart and vessels of circulation may be influenced by agents applied either to the brain or spinal marrow. It is as readily influenced by agents applied to the anterior part of the brain, as by those applied to the cervical part of the spinal marrow. This is what we should expect when we trace the origins of their nerves.

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* An account of these experiments is published in the introduction to the second part of my Treatise on Febrile Diseases, and a plate given representing the state of the vessels in the different stages of inflammation. See also the last part of this Inquiry.

If it be said that the results of these experiments imply a contradiction, that we cannot suppose the power of the heart and vessels to be wholly independent of the brain and spinal marrow, and yet influenced by agents applied to them, the reply is, that such are the facts, of the truth of which any one may easily satisfy himself.

On a closer examination of the phenomena of the nervous system, we shall find other similar difficulties. The experiments of M. le Gallois prove, in the most satisfactory manner, that a principal function of the spinal marrow is to excite the muscles of voluntary motion, and that it can perform this office independently of the brain. It performs it after the brain is wholly removed, and its powers seem not at all immediately impaired by the removal of the brain; yet we constantly see injuries of the brain impairing the functions of the spinal marrow. Of this apparent inconsistency, M. le Gallois justly remarks, that two facts well ascertained, however inconsistent they may seem, do not overturn each other, but only prove the imperfection of our knowledge.

Whichever of the disputed opinions respecting the functions of the nervous system, we adopt, the foregoing phenomena seem to imply a contradiction. The experiments related in the following chapter point out still another instance of this apparent contradiction, and seem to suggest the principle on which it as well as the others depend.

CHAP. III.

On the Principle on which the Action of the Muscles of voluntary Motion depends, and the relation which they bear to the Nervous System.

We are now to consider how far the principle on which the action of the muscles of voluntary motion depends, and the relation which they bear to the nervous system resemble those of the heart and vessels of circulation.

Exp. 31. By applying strong stimuli to the spinal marrow of a frog, strong and repeated contractions were excited in the muscles of the hind limbs, as long as the stimuli would produce the effect. On examining the state of the muscles of these limbs, I found them wholly deprived of their excitability. We should conclude from this experiment, that the power of the muscles of voluntary motion is wholly dependent on the nervous system; yet it is well known, that although all the nerves supplying the limbs of a frog be divided, and cut out close to the place where they enter the muscles, the latter still retain their excitability, which appears to be not at all less than when the nerves are entire. In the latter case it may be said, that the nervous influence, which was exhausted in the above experiment by stimulating the spinal marrow, still remains in the muscles after the nerves are divided, and thus preserves their excitability. The following experiment, therefore, was made.

Exp. 32. All the nerves supplying one of the hind limbs of a frog were divided, so that it became completely paralytic. The skin was removed from the muscles of the leg, and salt sprinkled upon them, which, being renewed from time to time, excited contractions in them for twelve minutes; at the end of this time they were found no farther capable of being excited. The corresponding muscles of the other limb, in which the nerves were entire, and of which consequently the animal had a perfect command, were then laid bare, and the salt applied to them in the same way. In ten minutes they ceased to contract, and the animal had lost the command of them. The nerves of this limb were now divided, as those of the other had been, but the excitability of the muscles to which the salt had been applied was gone. Its application excited no contraction in them. It sometimes happens, while the nerves of the limb are entire, that the voluntary efforts of the animal prevent the contractions usually excited by the application of salt. This experiment was repeated in the same manner, and with a similar result. After the experiment, the muscles of the thighs in both limbs were found to contract forcibly on the application of salt. It excited equally strong contractions on both sides.

It is remarkable, that in this experiment, the excitability of the muscles whose nerves were entire, was soonest exhausted. In the repetition of the experiment, this was the case to a still greater degree; the muscles, whose nerves were entire, losing their excitability in about one half of the time required for exhausting the other.

From this experiment it is evident, that the nervous influence, so far from bestowing excitability on the muscles, exhausts it like other stimuli. The excitability, therefore, is a property of the muscle itself. Yet we have just seen, that it may be wholly destroyed by changes induced on the nervous system. On the same principle we explain the seeming contradiction respecting the action of the heart and vessels. We have seen that their power exists as independently of the brain and spinal marrow, as the action of the first muscles to which the salt was applied, whose nerves had been divided; but, while the brain and spinal marrow retain their functions, and the connexion of nerves is entire, the heart and vessels, as well as the muscles of voluntary motion, may be influenced by agents acting through the nervous system. It is not difficult to account for the latter muscles being more copiously supplied with nerves than the heart, because all the stimuli which affect them, act through their nerves, while the heart is only now and then influenced through its nerves, its usual stimulus being as immediately applied to it, as the salt was to the muscles of the limb in the above experiment, and acting as independently of the nervous system. We do not surely, in the experiments which have been laid before the reader, see any difference in the nature of the muscular power of the heart, and that of the muscles of voluntary motion, except their being fitted to obey different stimuli, a difference which to a certain degree appears to exist in the two sides of the heart itself.

It may here be objected, that in apoplexy the power of the muscles of voluntary motion is lost, while that of the heart is often little or not at all impaired. Were such the fact, this objection would be unanswerable; but I have repeatedly examined the state of the muscles of voluntary motion in apoplexy, both in warm and cold-blooded animals, and found their excitability unimpaired. It is not their power, but the stimulus which excites them, that is lost in apoplexy. In this disease, the heart continues to contract, because its stimulus is still supplied; the muscles of voluntary motion cease to contract, because their stimulus is withdrawn *.

The conclusions afforded by the foregoing experiments so far agree with those of Haller, that they prove the heart and other muscles to possess an excitability independent of the nervous system; but they prove, contrary to the opinion of that great physiologist, that the heart is, equally with the muscles

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* See Chap. 10.

of voluntary motion, capable of being stimulated through this system.

In the report of the Institute of France, which has been laid before the reader, it is observed, " the adversaries of irritability have asked, why, if the nervous power has no action on the heart, is this organ supplied with nerves, and why is it subjected to the influence of the passions? Haller never gave any satisfactory explanation of these objections, but every thing proves that he felt all their force." These objections, we have seen, prevented Haller's doctrine of irritability from being generally admitted by physiologists, and at length led M. le Gallois to suppose that he had wholly refuted it.

We may, I think, trace the subject farther. It has been shewn by direct experiment by M. le Gallois, that the spinal marrow is capable of exciting the muscles, and we shall find it capable of its other functions independently of the brain, yet we constantly see the spinal marrow influenced through the brain. Thus the excitability of the spinal marrow bears the same relation to the brain, which that of the muscles bears to the spinal marrow and its nerves, and I would add all nerves distributed to muscles, some of which arise from the brain, but seem to bear precisely the same relation to the sensorium and to the muscles, with those which arise from the spinal marrow. Even M. le Gallois, although his experiments lead to an opposite conclusion *, observes, that the brain seems to act on the spinal marrow as the latter does on the parts it animates. We know the peculiar office of the brain, by observing what functions are lost by its removal, the sensorial functions. The nervous, then, obeys the sensorial system, in the same way in which the muscular obeys the nervous system; but as the muscular power has an existence independent of the nervous, so has the nervous an existence independent of the sensorial power.

I shall, towards the latter part of this Inquiry, endeavour to point out with more precision than has been done, the line of distinction between the sensorial and nervous functions, which appears to me, from direct experiments, to be very different from that assumed by M. le Gallois †.

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* He infers from his experiments that the power of the heart ceases on the destruction of the spinal marrow, but that that of the spinal marrow remains after the destruction of the brain,

† See Chap. 10.

CHAP. IV.

On the comparative Effects of Stimuli, applied to the Brain and Spinal Marrow, on the Heart and Muscles of voluntary Motion.

In making the experiments related in the preceding chapters it was evident, that although the muscles of involuntary are equally with those of voluntary motion subject to the effects of stimuli applied to the brain and spinal marrow, the laws which regulate these effects on the two sets of muscles are very different. The following experiments point out more precisely in what this difference consists.

Exp. 33. Part of the cranium of a rabbit was removed, and a wire passed in various directions through the brain. I could not in this way in the least affect the muscles of voluntary motion, except when I made the wire approach those parts of the brain from which the spinal marrow and nerves originate. Some of the muscles of voluntary motion were then thrown into violent spasms. I sliced off the whole of the upper and anterior part of the brain, without affecting the muscles of voluntary motion or apparently giving pain. The knife only excited their action when it approached the source of the nerves and spinal marrow.

Exp. 34. Having deprived another rabbit of sensibility and voluntary motion by a blow on the occiput, that I might be enabled to judge of the effects which a stimulus applied to the brain would produce on the heart, I removed part of the cranium and laid open the thorax. The heart was found beating regularly. By passing a wire through the brain in any direction, the beats of the heart were accelerated and rendered stronger. I could not perceive that this effect was produced more powerfully when the wire was directed towards the source of the nerves, than when any other direction was given to it, provided it passed through an equal portion of the brain. When an instrument was merely pressed on the surface of the brain, the effect was similar. When a pair of scissors, or any other thing of larger bulk than the wire, was passed into the brain, the effect on the heart was greater than from the wire. It was still greater when the brain was wounded rapidly in many directions.

Exp. 35. Part of the cranium of a rabbit was removed, and after passing a knife through the brain in various directions towards the origin of the nerves, which excited the strongest spasms in the muscles of voluntary motion, he blood being absorbed by a sponge, I applied strong spirit of wine to the surface of the brain, and dropt it into the cuts, without at all affecting the muscles of voluntary motion. The upper part of the brain was then wholly removed, and the space filled with strong spirit of wine, but no signs of pain nor spasms in the muscles of voluntary motion were excited.

Exp. 36. Another rabbit was deprived of sensibility and voluntary motion by a blow on the occiput. Part of the cranium was then removed, the thorax laid open, and the heart found beating regularly. Spirit of wine was now applied to the surface of the brain, by which the frequency and force of the heart's beats were immediately increased. Several cuts were then made in the brain, and the spirit of wine dropt into them, by which the action of the heart was increased in a much greater degree. Spirit of wine increased the action of the heart more than any mechanical injury, which never produced the strong action in this organ, that it does in the muscles of voluntary motion.

This experiment was repeated with a watery infusion of opium instead of spirit of wine; the result was in all respects the same, except that the action of the heart was less increased than by the spirit of wine.

Under the term brain, I mean to include the cerebellum as well as cerebrum. From many trials on rabbits made to ascertain the point, I could not perceive that the heart is more or less affected either by chemical or mechanical stimuli applied to the cerebellum than to the cerebrum; nor are the muscles of voluntary motion affected by wounding the cerebellum, except we approach the source of the spinal marrow and nerves. In some of my experiments, I thought that stimuli applied to the cerebellum affected the action of the heart rather more powerfully than when applied to the cerebrum; but this was contradicted by other experiments.

Exp. 37. I repeatedly cut off the head of a rabbit close to the occiput. For some time the trunk and limbs were affected with violent spasms. The cut end of the spinal marrow was so sensible that the slightest touch of a wire, after the spasms had subsided, immediately excited the action of some of the muscles of voluntary motion. The strongest spirit of wine and watery infusion of opium were applied to it, without producing the least effect on those muscles. The application, however, of stronger chemical stimuli, the nitric and muriatic acids, threw the muscles of the fore-legs into powerful contractions.

Having deprived a rabbit of sensation and voluntary motion, in an experiment already related, I found that both spirit of wine and a watery infusion of opium applied to the spinal marrow, increase the action of the heart.

Exp. 38. I found both in rabbits and frogs that, after all stimuli applied either to the brain or spinal marrow had ceased to produce any excitement in the muscles of voluntary motion, both chemical and mechanical stimuli so applied still increased the action of the heart; the former more than the latter.

Exp. 39. I tried, in every possible way, both by mechanical and chemical stimuli, and both before and after the sensibility was destroyed, to excite, through the brain or spinal marrow of rabbits and frogs, any irregular action in the heart which is so readily excited in the muscles of voluntary motion, but could not. Nor could I by sedatives, applied to the nervous system, occasion any irregular action in the heart. Its action was rendered quicker or slower, more or less frequent, stronger or weaker, but never irregular. Irregular action was never excited in the heart, except when its power was nearly destroyed by crushing the brain or spinal marrow.

Exp. 40. I found from many trials both on rabbits and frogs, that the excitement of the muscles of voluntary motion took place chiefly at the time the stimulus was applied to the brain or spinal marrow. It was generally necessary to move the instrument, thus applying it to a new surface, in order to support the effect. Repeated contractions of the muscles of voluntary motion will sometimes continue, assuming the form of a fit, as long as the instrument remains in the brain, although it be kept as still as the motions of the animal will admit of. The increased action of the heart, could generally be observed as long as the stimulus, whether chemical or mechanical, was applied, unless it was of a nature to produce the sedative after the stimulant effect. The sedative effect is so far from being the consequence of previous excitement, as many physiologists have supposed, that spirit of wine and mechanical stimuli, which produced no sedative effect, but continued to stimulate the heart as long as they were applied, produced a much greater degree of excitement than tobacco, whose slight stimulant effect was quickly succeeded by a powerfully sedative one.

It appears from these experiments, that chemical stimuli, applied to the brain and spinal marrow, exert a greater power over the heart than mechanical stimuli, while the latter exert a greater power over the muscles of voluntary motion than chemical stimuli; that both chemical and mechanical stimuli, applied to the brain and spinal marrow, excite the heart, after they cease to produce any effect on the muscles of voluntary motion; that stimulating every part of the brain and spinal marrow equally affects the action of the heart, while the muscles of voluntary motion are only excited by stimuli applied to the parts of those organs from which their nerves originate; that stimuli applied to the brain and spinal marrow never excite irregular action of the heart, while nothing can be more irregular than the action they excite in the muscles of voluntary motion; that their effect on these muscles is felt chiefly on their first application, but continues on the heart as long as the stimulus is applied. These differences in the effects of stimuli applied to the brain and spinal marrow on the muscles of voluntary and those of involuntary motion, must be explained before we can be said to understand the relation which subsists between the nervous system and the heart.

It appeared to me probable, from many experiments, that the cause of chemical stimuli, applied to the nervous system, producing a greater effect on the heart than mechanical stimuli do, is, that the former from their nature act on a larger portion of the brain and spinal marrow. If this opinion be correct, the mechanical stimulus will be rendered the most powerful by confining the chemical to a smaller space than the mechanical stimulus occupies.

Exp. 41. Both in frogs and rabbits I applied to various parts of the brain and spinal marrow, and particularly to those parts from which the nerves originate, minute portions of strong spirit of wine, without at all influencing the action of the heart. When these small portions were applied to a great many parts, the heart began to beat more frequently. This, of course, was much the same thing as at once applying the spirit of wine to a larger part. We have seen in the foregoing experiments, that mechanical stimuli applied to any considerable portion of the nervous system, increase the action of the heart. It appears from the following experiments that we cannot affect the heart by mechanical stimuli confined to any small part either of the brain or spinal marrow.

Exp. 42. In a rabbit, deprived of sensibility by a blow on the occiput, I wounded different small parts of the brain with a wire, particularly all those parts near which the nerves of the heart appear chiefly to originate; but could not affect the motion of this organ, while at the same time passing the wire through any considerable portion of the brain immediately accelerated it.

Exp. 43. I laid open the cervical part of the spine of a rabbit, rendered insensible by a blow on the occiput, and repeatedly passed a wire transversely through the spinal marrow, without being able at all to affect the motion of the heart; but on the wire being passed longitudinally, so as to bring it into contact with a larger portion of the spinal marrow, the motion of the heart was immediately accelerated. On the same principle, when the wire was made to wound many minute portions of the brain and spinal marrow in quick succession, the action of the heart was increased. In another rabbit, I divided the spinal marrow at the occiput without at all affecting the heart.

Mr. Clift, in an account of experiments on the carp, published in the Philosophical Transactions for 1815, observes, that on dividing the spinal marrow at the occiput, the action of the heart was greatly accelerated for a few beats; but he divided the spinal marrow while the animal retained the power of the muscles of voluntary motion, which never fail to be called into action, by wounding it, and whose action, by increasing the flow of blood, always accelerates the motion of the heart *.

Thus we see that neither chemical nor mechanical stimuli, applied to the brain and

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* It is particularly satisfactory to me that Mr. Clift, on repeating my experiment, in which the spinal marrow was destroyed by a hot wire, found the same result in the carp, which I had done in rabbits and frogs. He did not ascertain whether the circulation continued after the destruction of the spinal marrow, but from this occasioning little or no diminution in the action of the heart, we cannot doubt the continuance of the circulation.

spinal marrow, affect the action of the heart, unless they make their impression on a large portion of these organs. In the various experiments I have related, every part of them was stimulated individually, without the action of the heart being influenced; and the stimulus being the same, the force with which it acted on this organ was always proportioned to the extent of surface to which it was applied. I could not find that it was of any importance what part of the brain was stimulated. Even stimulating the surface alone, either mechanically or chemically, we have seen, immediately increased the action of the heart. The muscles of voluntary motion, on the contrary, it appears from the above experiments, are wholly insensible to stimuli applied to the brain, except near the origin of the nerves and spinal marrow. It is remarkable that while a rabbit perfectly retains its sensibility, and expresses great pain on any of the muscles being wounded, it exhibits no expression of pain from the brain being sliced, until the knife approaches the origin of the nerves or spinal marrow.

Another circumstance, which appears to be of great importance in tracing the cause of the different effects of stimuli applied to the brain and spinal marrow on the muscles of voluntary and involuntary motion, is, that the heart obeys a much less powerful stimulus than the muscles of voluntary motion do. We have seen that only the most powerful chemical stimulus affects them, while all that were tried readily influenced the action of the heart. Mechanical stimuli which, by bruising and dividing the parts, occasion the greatest possible irritation, are best fitted to excite the muscles of voluntary motion. Chemical stimuli, indeed, from their effects on the heart, we should consider the most powerful. But their greater effect on this organ is readily explained by the influence of stimuli applied to the brain and spinal marrow on the heart, being proportioned to the extent of surface to which they are applied. It is evident that the stimulus can be applied to a greater extent of surface in the fluid, than in the solid form. When the effect of the mechanical agent is rendered extreme, we find its influence on the heart far greater than that of any chemical agent I tried. From experiments related in the second chapter of this part, it appears, that suddenly crushing any considerable portion of the brain or spinal marrow instantly destroys the power of the heart.

The conclusions then at which we arrive are, that the heart is excited by all stimuli applied to any considerable part of the brain or spinal marrow, while the muscles of voluntary motion are only excited by intense stimuli applied to certain small parts of them.

These facts being ascertained, the other differences observed in the effects of stimuli applied to the brain and spinal marrow, on the heart and muscles of voluntary motion, are easily explained.

Irregular action of a muscle arises from stimuli acting partially, or at intervals, on its nerves, or on the particular part of the brain or spinal marrow, from which its nerves arise. But very partial action of a stimulus on the brain and spinal marrow, we have just seen, is incapable of exciting the heart, and while the stimulus is applied to any part of these organs, as all parts of them seem equally to influence the heart, it cannot act upon it interruptedly, as an instrument does on the muscles of voluntary motion when it is moved from place to place in the brain.

When the instrument is kept still after it is introduced into the brain, the action of the muscles of voluntary motion often ceases; its merely being in contact with the parts of the brain which excite these muscles, not being sufficient to call them into action. As the muscles of voluntary motion feel the impressions made on a very small part of the brain only, in proportion as this part is small, the impression must be great to affect them; but the heart, which is influenced through all parts of it, though not very powerfully through any one, feels all the impressions made on this organ, provided they are made on a sufficiently extensive portion of it; thus, within certain limits, as long as the instrument remains in the brain, its stimulant effect on the heart continues.

It is true, that although the heart is only influenced by agents applied to a large portion of the brain, we may conceive them so applied as to produce irregular action in it, and we find that certain irritations of the nervous system have this effect. But it is evident, that the heart not being subject to stimuli whose action is confined to a small portion of this organ, and being equally affected through all parts of it, must render it much less subject to irregular action; which may be one of the final causes of the heart, whose regular action is of such importance in the animal economy, being made subject to the whole, and not to any one part of the brain *; and readily accounts for our not being able to produce irregular action in it, in the above experiments.

What has been said also explains why those

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* In the course of this inquiry another and apparently more important reason will appear, why it is necessary that the heart should be subject to the influence of every part of the brain and spinal marrow.

who have endeavoured to influence the heart by stimulating the parts of the brain from which its nerves seem chiefly to originate, have failed. When indeed the source of the nerves of the heart is considered, it will be found to derive its nervous influence from every part of the nervous system, and not very remarkably from any one part; a circumstance which particularly corresponds with the result of the foregoing experiments.

From the same facts we explain, why the heart is stimulated through the brain and spinal marrow after their power is so far weakened as no longer to convey the effect of the stimulus to the muscles of voluntary motion. As these obey stimuli applied to only one part of those organs, if the change in this part is not sufficiently strong to produce the effect, it cannot be assisted by any other. Thus I have found by experiment, that a blow which affects the brain generally, without materially injuring it, produces comparatively little effect on the muscles of voluntary motion, because no one part suffers greatly; but it produces a great effect on the heart, because it feels the sum of all the impressions. The nervous system, therefore, may be so far exhausted as not to admit of the vivid impressions necessary to excite the muscles of voluntary motion, and yet capable of those which influence the heart.

CHAP. V.

On the Principle on which the Action of the Vessels of Secretion depends, and the Relation which they bear to the Nervous System.

It not only appears from the experiments which have been laid before the reader, that the power of the heart and vessels of circulation is independent of the nervous system, but that that of the muscles of voluntary motion is so likewise; and that these like the former are only subjected to this system in the same way in which they are subjected to every other agent that is capable of exciting them. Thus we find, that all the moving powers of the animal body, as far as we have hitherto traced them, are independent of the nervous system; but that this system is equally capable of affecting them, although in different ways, whether they are subject to the influence of the will or not. Is the power of secretion also independent of, though influenced by, the nervous system?

I was soon convinced, that although the powers of circulation are independent of the nervous system, those of secretion are very far from being so. M. le Gallois, in the treatise so frequently alluded to, enumerates many physiologists who divided the eighth pair of nerves; and he gives a minute account of the consequences of their division, particularly of those which he himself observed in rabbits and guinea-pigs. The chief are, oppressed breathing and loss of power in the stomach. Mr. Brodie also gives an account of experiments, in which he divided the eighth pair of nerves in dogs, in a paper published in the Philosophical Transactions for 1814. But the animals always died of oppressed breathing before he could judge of the effect on the stomach. He proved, however, by another set of experiments, that arsenic introduced into the system after the division of the eighth pair of nerves, does not produce the copious secretion from the stomach and intestines which it is found to do under ordinary circumstances. He found a similar result when he divided the stomachic nerves immediately above the cardiac orifice of the stomach.

The lungs are affected differently according to the part at which the nerve is divided. If, in the rabbit, it be divided before the inferior laryngal nerve is sent off, or this nerve itself is divided, great difficulty of breathing, with a croaking noise, immediately follows;

arising, as M. le Gallois has shewn *, from the opening of the glottis, becoming much narrowed as soon as the nerve is divided. If the eighth pair of nerves are divided below the place at which they send off this nerve, there is little or no dyspnœa for some time. Mr. Hastings, who watched the progress of the following experiments with great care, observed, that when the eighth pair of nerves were divided below the inferior laryngal nerve, the dyspnœa, although it often came on sooner, was greatly increased by the attempts to vomit, which generally happened almost immediately after eating ; but which, if the animal was not allowed to eat, frequently did not occur for an hour and half or two hours after the division of the nerves. The dyspnœa increases, and the animal seems at last to die of it. The lungs are found after death distended with a frothy fluid, which fills the bronchia and air-cells, and prevents the lungs collapsing ; and they are covered with patches of a dark red colour, often of great extent, which give the

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* M. le Gallois says, that the difficulty of breathing comes on from dividing the recurrent nerves ; but Mr. Hastings, who frequently performed the experiment, always found that there was little or no dyspnœa induced in the rabbit by dividing the recurrent nerves. It was when he divided the laryngal nerves that the sudden dyspnœa, mentioned by M. le Gallois, took place.

appearance of the vessels being greatly distended with blood, or of blood having escaped into the cellular substance. They appear of a more compact texture than healthy lungs, and sink in water. In short, the fluids of the lungs no longer undergo the proper change, but accumulate in the bronchia, air-cells and blood-vessels. As M. le Gallois' account of the effects of the division of these nerves on the stomach does not altogether agree with that of the authors he quotes, and is also in other respects contradictory, Mr. Hastings frequently repeated the experiment on rabbits, and sometimes with such dexterity, that the animal lived about twenty-six hours after the operation. I had thus an opportunity of ascertaining, that even during this length of time, although the stomach was full of food (parsley), no change on it had been effected. It continued in the same state as when it left the mouth, simply divided by mastication, preserving perfectly both its appearance and its smell. It was impossible to distinguish it from parsley chopped small with a knife.

It occurred to me, that the pain and irritation occasioned by the operation might in these experiments have induced such a degree of disease as to destroy the powers of digestion, independently of any specific effect on the stomach. I, therefore, requested Mr. Hastings to perform the experiment in the following manner.

Exp. 44. Two rabbits, of about the same age, were fed in the same way. In both the eighth pair of nerves were laid bare. In the one rabbit they were divided; in the other, after being raised on a probe, they were replaced without injuring them. Both rabbits were allowed to eat as much parsley as they chose after the operation. When that in which the nerves were divided died, which did not happen for more than twenty hours after the operation, the other was killed. In the former the food was found wholly undigested; in the latter the digestive process had gone on as usual, and the food was found in the same state as in a healthy rabbit.

The stomach is generally distended to a greater size than usual, when the eighth pair of nerves have been divided, the cause of which will hereafter appear. This happened in the present case. It is remarkable, that the œsophagus also is found to contain food, and is often very much distended. From these circumstances, and from an experiment of M. le Gallois, in which one only of the eighth pair of nerves being divided in a guinea-pig, the animal survived several days, and the stomach became enormously distended with undigested food; I say, from these circumstances it occurred to me, that the sensation by which an animal judges when he has received enough of food, being destroyed by the division of the nerves, the animals had perhaps occasioned over-distension of the stomach, and thus destroyed the power of digestion, for they often ate a great deal after the operation. I therefore requested Mr. Hastings to repeat the experiment, allowing the animal to eat as much as he chose before the operation, but none after it.

Exp. 45. This he did, but the result was the same. The food with which the rabbit had filled its stomach just before the division of the nerves remained wholly unchanged; and it was remarkable, that the œsophagus was just as much distended with the food as when the animal had eaten after the operation. This arises from the fruitless efforts to vomit, which always come on in an hour or two after the division of the nerves.* It deserves notice, that although the eighth pair of nerves have been divided, the food is found covered with apparently the same semifluid, which we find covering the food in a healthy stomach.

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* I have already mentioned that Mr. Hastings observed the dyspnœa greatly increased by the fruitless attempts to vomit. As the œsophagus in the rabbit lies contiguous to the yielding part of the trachæa, the distension of the former cannot fail to lessen the capacity of the latter.

These experiments leave no room to doubt, that the office of the stomach is suspended by dividing the eighth pair of nerves. A similar observation applies to the lungs.

In the animal in which the eighth pair of nerves was merely raised on the probe, the lungs continued perfectly to perform their office, and were found of a healthy appearance after death. In all the instances in which these nerves were divided, great dyspnæa, we have seen, soon came on, and the air-cells and tubes were found clogged with frothy mucus.

It appears then, that the extreme parts of the sanguiferous and nervous systems are connected in a way very different from that in which these systems are connected in other parts. The heart and vessels of circulation, we have seen, can perform their functions after the nervous influence is withdrawn. The power of secretion immediately ceases on the interruption of this influence. We must suppose, therefore, either that the nervous influence bestows on the extreme vessels the power of separating and re-combining the elementary parts of the blood, or that the vessels only convey the fluids to be operated upon by this influence.

Experiments, related in the second chapter of the present part of this Inquiry, shew that the most minute vessels which can be seen by a powerful microscope in the web of a frog's foot, are independent of the nervous system. The motion of the blood is as rapid, and the circulation in the foot presents precisely the same appearance after, as before the destruction of that system. Is it consistent with these experiments to suppose that any part of the sanguiferous, derives its power from the nervous, system? If the power of the vessels of secretion had been lost by the destruction of the latter, would not this have occasioned some change in the distribution and motion of the blood in the web? The conclusion from these experiments is strengthened by other facts. In those experiments in which the power of secretion was destroyed by withdrawing the nervous influence, there appeared to be no defective supply of fluids. Both in the stomach and in the lungs they were sufficiently copious. The fault seemed to be, that a due change on them had not been effected. We have no reason to believe, as far as I am capable of judging, that the vessels possess any other than a muscular power, if we except the mere power of elasticity*. Now we have seen it proved, by direct experiment, that the muscular power throughout the whole animal, namely, in the muscles of voluntary motion, the heart and the vessels of circulation, is independent of the nervous system. Can we suppose that the vessels of secretion, which are only a continuation of those of circulation, all at once assume a different nature? Or, is it at all consistent with our knowledge of the phenomena of chemistry, to conceive that, by any influence, the muscular power can be enabled to separate and re-combine the elementary parts of the blood?

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* I do not mean here to enter on the arguments which seem to prove that the power of the vessels is, strictly speaking, a muscular power. For this subject I refer the reader to the observations of Mr. Hunter, in his work, entitled, A treatise on the Blood, Inflammation, and Gun-shot Wounds. In some

The first of the above positions, therefore, may, I think, be regarded as set aside. This admitted, does it not seem a necessary inference from the preceding experiments and observa-

animals the muscular structure of the vessels is apparent on the slightest view. In man, and animals resembling him, it is very obscure, the reason of which Mr. Hunter has pointed out. In them the proportion of the muscular to the elastic coat of the vessels is inversely as the size of the vessels; so that in the larger vessels there is comparatively little muscular fibre, and in the vessels possessing a larger proportion of it, the parts are too minute to enable us to detect it. The arguments of Mr. Hunter are surely much strengthened by the foregoing experiments, which shew, that the laws which regulate the power of the vessels of circulation are the same with those which regulate that of the heart, whose muscularity cannot be questioned.

The experiments of Burzelius and Dr. Young afford the strongest argument against the muscularity of vessels, by shewing that fibrine is not discoverable in their coats.

tions, that in the function of secretion the vessels only convey the fluids to be operated upon by the nervous influence?

Thus we have every reason to believe, that the vessels of secretion, like those of circulation, are independent of the nervous system; secretion failing when the influence of this system is withdrawn, not because the vessels of secretion fail to perform their office, but because the necessary changes on the fluids which they supply no longer take place.

We know that the nervous power occasionally influences the vessels of secretion, as we have seen it does those of circulation, because affections of the mind frequently occasion an increased flow of fluids to secreting surfaces. The vessels of secretion, therefore, thus far obey the same laws as those of circulation: they are independent of, but influenced by, the nervous power. Are they influenced by certain parts of the brain alone, or, like the heart, by every part of the brain and spinal marrow? To save repetition this question will be considered, with another intimately connected with it, in the second section of the seventh chapter.

It is not to be overlooked, that the vessels convey the fluids, to be operated upon by the extreme parts of the nervous system, in a peculiar way. By the lessening capacities of the capillary vessels, the blood is divided as by a fine strainer, some of its parts being too gross to enter the smaller vessels. How far the blood may thus be subdivided we cannot tell. As this structure of the vessels is uniform, we have reason to believe that its effect on the blood is necessary to prepare this fluid for the due action of the nervous influence.

CHAP. VI.

On the Principle on which the Action of the Alimentary Canal depends; with some Observations on an Opinion of Mr. Hunter.

In order to ascertain how far the peristaltic motion of the intestines is independent of the brain and spinal marrow, the following experiments were made.

Exp. 46. A rabbit was deprived of sensibility by a blow on the occiput. The whole of the spinal marrow was then destroyed by a hot wire. On opening the abdomen we found the peristaltic motion of the stomach and small and great intestines quite as strong as when the nervous system is entire, as we ascertained by exposing the abdominal viscera of other newly-dead rabbits. This motion is as strong in the newly dead, as in the living animal.

Exp. 47. The spinal marrow was wholly removed in another rabbit, also deprived of sensibility by a blow on the occiput, without at all affecting the motion of the stomach and intestines. The removal of the brain we found produces as little effect upon it, as that of the spinal marrow. When both were removed at the same time it remained unaffected. It continues till the parts become cold, so that when the intestines exposed to the air have lost their power, that of those beneath still remains.

It appears from these experiments, that the muscular power of the stomach and intestines, like that of the heart and blood-vessels, resides in themselves, and is wholly independent of any influence derived from the nervous system. Thus we see that the muscular fibre here, as in every other part in which we have examined it, appears to be independent of this system.

Mr. Hunter, in a work just referred to, has proved the vitality of the blood, in so clear and convincing a manner, that although the prejudices of his opponents may still lead them to object to his arguments, it is impossible to reply to them. This doctrine has been slowly received, because those, who have not been in the habit of correcting by reflection, the ideas received from first impressions, constantly associate their ideas of life with those of sensation and voluntary power. To such, Mr. Hunter's illustration of his opinion by the process of incubation, will be the best reply. We see nothing in the egg, on a superficial view, but glairy fluids contained in membranes: the blood itself is not more unlike what is usually called life: yet who will doubt the life of an egg, when he sees that if it be merely kept in the proper temperature for three or four weeks, an animal of the most perfect kind, complete in all its parts, comes out of it?

Mr. Hunter has shewn, by decisive experiments, the analogy which subsists between the contraction of a muscle and the coagulation of the blood. The two acts in many respects obey the same laws. Now the muscular fibre is formed chiefly from that part of the blood which coagulates. The coagulation of the blood every one will allow to be independent of the nervous system. It takes place at least as readily out of the body as in it, and no nervous filaments pass into the blood. In the formation of this part of the blood into muscular fibre, therefore, it must wholly change its laws if its power now depends on the nervous system; and we can hardly suppose that the analogies, pointed out by Mr. Hunter, could in this case have existed.

One of these analogies is intimately connected with the result of some of the preceding experiments. It was shewn in the second chapter, that the power of the muscular fibre may be destroyed by the most powerful agents acting on the nervous system. Mr. Hunter observes, that as the muscles, in instantaneous death from passion, blows on the stomach, or electricity, do not contract after death, so the blood under the same circumstances is found not to coagulate. When the latter fact is compared with the experiments just alluded to, it appears a necessary conclusion, that the nervous influence is capable of acting on the blood.

The inability of the muscle to contract, and of the blood to coagulate under the foregoing circumstances, Mr. Hunter ascribes to the want of strong action in the muscles and blood where death is so sudden. But it appears from experiments which have been laid before the reader, that strong action, previous to death, tends to destroy the excitability of the muscles, which is always found to remain, as I have ascertained by many experiments, if they have for some time previous to death been at rest, unless it is destroyed by the instantaneous destruction of the nervous power, as in the cases just mentioned. I have found that even the presence of opium in the system, only exhausts the excitability of the muscles in proportion to the frequency and force of the contractions it excites in them *. Compare Exp. 19, 20, 21, and 22, with Exp. 31.

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* The convulsions excited by opium always assume the form of the opisthotonos. The animal becomes rigid and is bent backwards. During the intermission of these convulsions the slightest touch renews them. If the animal is allowed to remain undisturbed they but rarely occur. By frequently touching it, a constant succession of them may be kept up. In the former case the excitability of the muscles is little impaired, in the latter case it is nearly exhausted.

As I have frequent occasion in this Inquiry to mention the effects of opium and tobacco, I shall take the present opportunity of laying before the reader the result of many experiments which I made with a view to ascertain the *modus operandi* of these drugs on the living animal.

It appears from these experiments that their effects may be divided into three classes.

1st. Their effects on any part of the body to which they are immediately applied. In very small quantity they tend to excite muscular action. In larger quantity they immediately destroyed the muscular power. They produce these effects in the hollow muscles, the heart, intestines, &c., chiefly when applied to their internal surfaces. When injected in considerable quantity under the skin, they destroy the circulation in the part by destroying the power of its vessels. By their operation on the nerves of the part, when applied in small quantity, they produce a degree of excitement in the whole nervous system, and through it in the sanguiferous system. In large quantity, especially when applied to a very sensible and extensive surface, they produce to a greater or less degree an immediate torpor of the nervous, and through it of the sanguiferous, system. In

Of the physiologists, either of our own or former times, none ranks higher than Mr. Hunter. In originality he has perhaps had no equal:

all these instances the stimulant effect of the opium is more considerable than that of the tobacco, and the sedative effect of the latter is more considerable than that of the former.

The second class of the effects of opium and tobacco are those which they produce on the sanguiferous system in consequence of their absorption. These effects begin in about a quarter of an hour or twenty minutes after they are received into the stomach and intestines, and appear to be precisely the same as the effects of opium and tobacco when immediately applied in small quantity to the heart and blood-vessels; but here they are complicated with the third class, into which I would divide the effects of these drugs on the living animal, namely, their effects on the brain.

These are some degree of excitement, most remarkable when opium is used, followed, except the quantity be very small, by languor and an inclination to sleep, the former most considerable from tobacco, the latter from opium; and if the quantity be great, by general convulsions of the kind just described when opium has been used, and when tobacco has been used, by tremblings and paralysis. I have made many experiments to ascertain whether the last effects can arise from the action of opium and tobacco on any other part than the brain, and have found that they never arise from the action of those drugs on any other part. It has been maintained by Dr. Monro, and others, that convulsions arise from the action of opium on the nerves of the heart, because they found on injecting an infusion of opium into the heart, that convulsions very quickly ensue. But this only happens in consequence of the infusion passing along the aorta to the brain, for if the aorta be secured by ligature no convulsions take place. The yet I have hitherto had little occasion to mention his name, and in the Report of the Institute of France, which has been laid before the reader, his name is not mentioned at all. This, which seems at first view to throw some reflection on the importance of Mr. Hunter's labours, is in fact his highest praise. It arises from his having, in his principal physiological investigations, struck into a wholly new field of inquiry, so that in examining the opinions of others, we are hardly led to any mention of his. The same circumstance has occasioned

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same observations I have found by experiments frequently repeated, apply to the tremblings and paralysis produced by tobacco. Now as these effects of tobacco and the convulsions produced by opium always sooner or later ensue to whatever parts of the body they are applied, the experiments above alluded to prove that they are received into the system by means of the absorbents.

It would enlarge this note to the size of a treatise to give a detail of all the experiments from which the above inferences were drawn; for the present, therefore, I must beg the reader to believe that they were performed with all requisite care. No inference was drawn from any experiment till it had been frequently repeated with the same result. Many of these experiments were detailed in an appendix to the third volume of the second edition of my Treatise on Febrile Diseases. This and another appendix, being a set of experiments made with a view to ascertain the circumstances in diet which influence the spontaneous depositions from the urine, were omitted in the last edition, in order that the work might be comprised in two volumes.

his opinions to be but little known abroad. While the attention of physiologists was chiefly directed to ascertain the nature of that power on which muscular action depends, and the relation of the muscular and nervous systems; and while in these subjects there were still ample fields of dispute, they were not likely to be diverted from them to inquiries, apparently unconnected with their favourite objects.

Previous to Mr. Hunter, the physiologists of this country joined their endeavours to those of the continent in the above researches. From his childhood he was averse to the restraints of education, and he at no time bestowed much pains on acquiring a knowledge of the opinions of others. In the volume of nature he found the work best suited to his taste. It lay open before him, and uninfluenced by the habits of his predecessors, it was not surprising that he should turn to a page different from that on which they had dwelt so long. Thus he gave a new turn to the pursuits of British physiologists, which has ever since distinguished them from those of the continent. While the latter were occupied in endeavouring to establish or refute the opinions of Haller, and to add to the valuable facts ascertained respecting the subjects of these disputes; the former have in the same way been engaged in endeavouring to support or refute the opinions of Mr. Hunter and to extend our knowledge of the subjects to which he had directed his attention.

The advancement of science will necessarily draw together these different sects of physiologists, and the objects of their pursuit will at length become the same. As they acquire a more perfect knowledge of the animal economy, each sect will perceive that the discoveries of the other are necessary for the advancement of their own, and the endeavours of all will thus be directed to one end. Mr. Hunter's name, we may venture to predict, will then stand as high in other countries as it does in his own. I do not mean to say, with the blind admirers of Mr. Hunter, that we know nothing but what he has taught us, and that foreign physiologists might have learnt from him what they have learned from their own labours. He has not laboured in the same field with them; he has broken new ground, in the cultivation of which they will join; he has opened new views, for which they will make their acknowledgments, as soon as the determination of the questions, by which they have been so long occupied, gives them leisure to view what he has done in another and not less important part of the same subject*.

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* I say nothing here of the great pathological labours of this acute observer. There are two peculiarities of Mr. Hunter's

We are now to inquire whether the alimentary canal, like the heart and blood-vessels, is capable of being stimulated through the nervous system.

CHAP. VII.

On the Relation which the Alimentary Canal bears to the Nervous System.

The alimentary canal is of such importance in the animal economy, that it is of the first consequence, in tracing the laws of the vital functions, to ascertain the principle on which its action depends, and the relation which subsists between it and the nervous system; the former of these points I have endeavoured to

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writings, which will tend to prevent their becoming popular with foreigners; an occasional obscurity of style, and a degree of refinement which often loses sight of all correct rules of reasoning; an example of the latter we have in his doctrine of the stimulus of necessity.

The reader will not be surprised to find in the works of Mr. Hunter, repetitions and other marks of their often having been composed in haste, and under the pressure of many engagements. These, while they lessen the accuracy of the works, tend perhaps rather to raise our opinion of their author, for he is never betrayed into a neglect of the great lines of his subject, or inaccuracies of consequence.

ascertain by the experiments related in the preceding chapter, the latter we are now to consider.

Exp. 48. I endeavoured, in the newly-dead animal, to ascertain how far the motion of this canal is influenced by stimuli applied to the brain and spinal marrow; but from its nature it is in every way so irregular that no certain result could be obtained. It often appeared that spirit of wine, applied to the brain and spinal marrow, increased it.

The admission of air into the cavity of the abdomen throws the bowels into strong spasmodic action, which alone would obscure any effect that can be supposed to arise from stimulating the brain. The abdomen of the newly-dead animal was therefore opened under tepid water, but this was found to excite even stronger spasms than the air had done.

The effects of the passions on the alimentary canal, however, leave no room to doubt that it is capable of being stimulated through the nervous system. It remains to be ascertained whether it is subject only to certain parts of the brain, or like the heart, to every part of that organ and of the spinal marrow.

It is evident, from the circumstances just mentioned, that it is impossible to answer this question respecting the alimentary canal, as respecting the heart, by agents applied to different parts of the brain and spinal marrow. Before I relate the experiments which I had recourse to for this purpose, I shall make some observations on the process of digestion in the animal on which these experiments were made, which will place in a clearer point of view both their results, and those of some experiments already laid before the reader.

SECT. I.

On the Process of Digestion.

On the functions of the stomach all other functions of the animal body may be said to depend, as their various organs derive from it that supply, without which they can exist only for a very short time. In another point of view we find the stomach equally important. There is no other organ whose diseases are at once so frequent and so varied, or which partakes more, perhaps so much, of the diseases of other parts, or of the whole system.

It is not, however, my intention to enter fully into that part of the process of digestion which is performed in the stomach. The experiments of Spalanzani and others sufficiently prove that the change which the food undergoes in this organ is effected by a fluid secreted by it. I shall confine myself to such circumstances attending this change as serve to elucidate the results of the experiments which have been or are about to be laid before the reader.

I have inspected after death, under various circumstances, and at different periods after taking food, the stomachs of about a hundred and thirty rabbits, which has enabled me not only to ascertain some points that will place the result of many of my experiments in a clearer light, but to observe more particularly than has been done by others, the process of digestion in this animal; which, from the ease with which it can be procured, and its tenacity of life, has been so much employed in physiological investigations. How far the following observations will apply to other animals I cannot say. In carnivorous animals the process of digestion may be different, and in animals furnished with a gizzard in some respects it certainly is so. A knowledge of this process in any one animal, however, must be useful in our attempts to trace it in others.

The experiments on this part of the subject were so frequently repeated, that it would be tedious and unprofitable to give an account of each experiment. I shall here, therefore, under the head experiment, give the result of all the experiments on each particular part of the subject. Mr. Sheppard was so good as to assist me in these experiments.

Exp. 49. The first thing which strikes the eye on inspecting the stomachs of rabbits which have lately eaten, is, that the new is never mixed with the old food. The former is always found in the centre, surrounded on all sides by the old food, except that on the upper part, between the new food and the smaller curvature of the stomach, there is sometimes little or no old food. If, as we ascertained by more than twenty trials, the old and the new food are of different kinds, and the animal is killed after taking the latter, unless a great length of time has elapsed after taking it, the line of separation is perfectly evident, so that all the old may be removed without disturbing the new food. For this purpose we fed rabbits on oats, and, after making them fast for sixteen or seventeen hours, allowed them to eat as much cabbage as they chose, and killed them at different periods, from one to eight hours, after they had eaten it.

On opening the stomachs of rabbits three or four weeks old, who both sucked and eat green food, we always found the curdled milk unmixed with the green food. Before the stomach was opened we could, from its transparency, see where the green food and where the milk lay. The rabbits, used in this and all the experiments which I am about to relate in this section, were killed by a blow on the occiput.

Exp. 50. If the old and the new food be of the same kind, and the animal is allowed to live for a considerable time after taking the latter, the gastric juice, passing from the old to the new food, and changing as it pervades it, renders the line of separation indistinct, so that on a cursory view we should suppose the old and new food mixed together; but towards the small curvature of the stomach, and still more towards the centre of the new food, we find it, unless it has been very long in the stomach, comparatively fresh and undisturbed. All around, the nearer the food lies to the surface of the stomach, the more it is digested. This is true even with regard to the food in the small curvature, compared with that nearer the centre, and the food which touches the surface of the stomach is more digested than any other found in the same part of the stomach. But unless the animal has not eaten for a great length of time, the food in contact with the surface of the stomach is in very different stages of digestion in different parts of this organ. It is least digested in the small curvature, more in the large end, and still more in the middle of the great curvature.

These observations apply to the cardiac portion of the stomach. Sir Everard Home, in his work on Comparative Anatomy, has shewn that the stomach is divided into two portions, the cardiac and pyloric, in such a way, that the length of the former is to that of the latter nearly as two to one. The line of division may generally be seen in some animals after death. He says it is more evident while digestion is going on. I have sometimes observed it very distinctly after death in the stomach of the rabbit, and have then found the food in the two cavities divided by an evident line of separation as described by this author. These two portions of the stomach form an angle with each other, which is well expressed by the plates in Sir Everard Home's work.

Exp. 51. The food in the pyloric portion of the stomach of the rabbit, is always found in a state very different from that just described. It is more equally digested, the central parts differing less in this respect from those which lie next the surface of the stomach; it is evident, however, that all the change effected in the stomach is not completed when the food enters this portion of it, because we find it the more digested the nearer it approaches to the pylorus, where being ready to pass into the intestine, it has undergone all that part of digestion which is performed in the stomach.

One of the most remarkable differences between the state of the food in the cardiac and pyloric portions of the stomach, is, that in the latter it is comparatively compact and dry, in the former mixt with a large proportion of fluid, particularly when digestion is pretty far advanced, and time consequently has been given for a considerable secretion from the stomach. In the rabbit, indeed, which is fed only with solid food, in the early stage of digestion it is nearly as free from liquid in the cardiac as in the pyloric portion of the stomach. When digestion is very far advanced, the whole contents of the former are often reduced to the state of a semi-fluid. But even then the contents of the pyloric portion, particularly those parts which are near the pylorus, are comparatively compact and dry. In rabbits so young as to live wholly on milk, the curdled milk is considerably softer and moister in the cardiac than in the pyloric end of the stomach. An interesting question here arises: What becomes of the liquid part of the contents of the cardiac portion of the stomach when the solid part is moved on towards the pylorus?

This question has particularly engaged the attention of Sir Everard Home. What first suggests itself is, that as the stomach is constantly secreting a fresh supply of fluids, for the purpose of digestion, those which have performed their office, and are no longer useful in this cavity, are removed by absorption, analogous to what appears to be constantly going on in other parts of the body. But Sir Everard Home relates several experiments, from which he infers, that liquids are removed from the cardiac portion of the stomach by some other means than the absorbent system. What these means are he found it impossible with certainty to determine.

Exp. 52. Although the food is in the most digested state in the pyloric end, it appears from several circumstances that the change is chiefly effected in the great end of the stomach. The food found in the pyloric end we have just seen is comparatively dry, while that found in the great end, if digestion is much advanced, is mixed copiously with the juices of the stomach, and there is a more evident difference in the state of the food before it comes into this part, and when it is about to leave it, than in any other part of the stomach. I shall presently have occasion to mention a fact ascertained by Mr. Hunter, which seems to confirm this opinion. Mr. Hastings, on examining the stomach of a woman who had died under his care, found it every where in a state of ulceration, except in the great end, where it was healthy. The stomach had performed its functions to the last, and the fæces proved that the food had been properly digested.

It appears that in proportion as the food is digested, it is moved along the great curvature, where the change in it is rendered more perfect, to the pyloric portion. Thus, the layer of food lying next the surface of the stomach is first digested. In proportion as this undergoes the proper change, it is moved on by the muscular action of the stomach, and that next in turn succeeds to undergo the same change. As the gastric juice pervades the contents of the stomach, though apparently in no other way than by simple juxtaposition, (for the arrangement of the food, above described, we never found disturbed), the change in each part, which in its turn comes in contact with the stomach, is far advanced before it is in actual contact with it; and, consequently, is soon after this in a proper state to be moved on towards the pyloric end. Thus a continual motion is going on, that part of the food which lies next the surface of the stomach passing towards the pylorus, and the more central parts approaching the surface. Whether food is ever so digested in the small curvature, as to be sent to the pyloric portion, without having traversed the large curvature, I have not been able to ascertain. When rabbits have fasted sixteen or eighteen hours, the whole food found in the cardiac portion, which is in small quantity compared to what is found in it immediately after a full meal, seems frequently to be all nearly in the same state with that next its surface, the gastric juice having pervaded and acted upon the whole, and is consequently apparently fitted to be sent to the pyloric end. Sir Everard Home found that the stomach of a rabbit never empties itself, containing, even when the animal dies after long fasting, a considerable quantity of food. The first impression on the food is made in the small curvature, because the upper part of the new food, which lies contiguous with this part of the stomach, or nearly so, is always found more changed than the more central parts of it.

We frequently found in the large end of the stomach the small round masses or balls, about the size of the largest kind of shot, mentioned by Sir Everard Home. These balls are very constantly found in the great end of the stomach of rabbits, especially when fed on green food, never in any other part of it. They are often very numerous, sometimes forming the whole contents of that part of the stomach. They cannot be fewer, in many cases, than from two to four hundred. At other times they are much less numerous, and mixed with food of the same consistence with that of which they are composed. It is difficult at first view to account for their appearance. The ingenious idea of Sir Everard Home, that they are produced by the rabbit occasionally ruminating, is opposed by several circumstances; the frequency of their appearance, their sometimes forming one half or more of the contents of the stomach, their being always found at a considerable distance from the opening of the œsophagus into the stomach, unless their number is so great as to fill the greater part of the stomach, food much less digested than that composing them generally lying between them and this orifice, and no appearance of this kind being found in ruminating animals.

It was long before I could form even a probable conjecture respecting the formation of these balls. I have now, from inspecting many stomachs containing them, very little doubt of the cause to which they are to be ascribed. When the stomach of the rabbit is laid open, the great end is found corrugated forming rugæ, which give it a honey-comb appearance. These rugæ disappear when it is stretched, and as soon as the stretching power is withdrawn, again appear, the surface of the rest of the stomach being comparatively even. The balls seem to be formed in the hollows of these rugæ, which are about the same size with the balls. It would appear that the food, by the action of this part of the stomach, is rolled up into these masses after it has undergone that part of the digestive process which takes place in the great end of the stomach, and consequently after it has been exposed for a considerable time to the action of the gastric juice; in which form it is sent towards the pyloric end, where the balls are broken down, and the whole again formed into one mass of a firmer consistence than the balls. I have observed that when all the food in the great end of the stomach is composed of these balls, it contains no fluid but that which is mixed up with the food in them. Sometimes no balls are formed. This is comparatively rare. We never found the curdled milk formed into balls, consequently there are none in the stomachs of very young rabbits. With this exception they are frequently, I may say very generally, found under all circumstances of diet, situation, &c. Sometimes when rabbits had lived precisely in the same way, they were not found in all. They are sometimes found, when the more central parts of the contents of the stomach have undergone little or no change.

It would appear from several observations, that when the whole of the contents of the great end of the stomach have equally undergone the action of the gastric juice, so that there is no fresher food in the central parts, they are very slowly passed into the pyloric portion. Hence I conceive it arises, that they are often, when the animal has fasted for a considerable time, wholly formed into balls; when it has fasted for a great length of time, so much fluid is secreted in the great end of the stomach, that the balls lose their consistency, and the whole runs into a uniform semi-fluid mass.

Exp. 53. It is in the great end of the stomach where digestion appears to go on so rapidly, that Mr. Hunter found the stomach itself dissolved; and by the most satisfactory arguments shewed that this is the effect of the gastric juice after death. His observations on this subject confirm the foregoing view of digestion, for he found part of the stomach digested when the food it contained remained undigested, in the case of a man killed immediately after a full meal. This I have often observed in rabbits, when the animal has been killed immediately after eating, and allowed to lie undisturbed for some time. On opening the abdomen we have found the great end of the stomach soft, eaten through, sometimes wholly consumed, the food being only covered by the peritoneum, or lying quite bare for the space of an inch and a half in diameter, and part of the contiguous intestines, in the last case, also consumed; while the cabbage, which the animal had just taken lay in the centre of the stomach unchanged, if we except the alteration which had taken place in the external parts of the mass it had formed, in consequence of imbibing gastric juice from the half-digested food in contact with it. We sometimes found the great end of the stomach digested within an hour and a half after death; it was more frequently found so when the animal had lain dead for many hours. The great end of the stomach is not always dissolved however long the animal has lain dead. This seems only to take place when there happens to be a greater supply than usual of gastric juice. Thus we always observed it most apt to happen when the animal had eaten voraciously. Why it should take place without the food being digested is evident from what has been said. Soon after death the motion of the stomach, which is constantly carrying on towards the pylorus the most digested food, ceases. Thus, the food, which lies next to the surface of the stomach, becoming fully saturated with gastric juice, neutralizes no more; and no new food being presented to it, it necessarily acts on the stomach itself, now deprived of life; and on this account, as Mr. Hunter justly observes, equally subject to its action with other dead animal matter. It is remarkable that the gastric juice of the rabbit, which in its natural state refuses animal food, should so completely digest its own stomach, as not to leave a trace of the parts acted on. I never saw the stomach eaten through except in the large end. In other parts its internal membrane is sometimes injured.

Keeping in view the foregoing account of the process of digestion in the rabbit, it will be interesting to trace the effect produced on it by depriving the stomach of a great part of its nervous influence by dividing the eighth pair of nerves.

The division of the eighth pair of nerves, which I have so frequently had occasion to mention, is one of the oldest physiological experiments of which we have any account. It was performed by several of the ancients, and has been repeated by a great many physiologists in modern times. Valsalva is among the first who gave any distinct account of its effects on the stomach. He observes that it impedes digestion, and even prevents the food passing from the œsophagus into the stomach. The cause of part of the food being found in the œsophagus, I have had occasion to point out above. Haller frequently repeated this experiment, and observes that the powers of digestion were always suspended by it. Since his time it has often been made by others with the same result*.

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* It is said that M. Magendie has divided the eighth pair of nerves immediately above the diaphragm, and found that the stomach is still capable of performing its functions. Of the effects of the division of the eighth pair of nerves at this place I cannot speak, as I have never seen the experiment made. Its effects on the stomach, it is evident, may be different from that

If the animal be allowed to live for a considerable time after the division of these nerves, the food remaining in the stomach is always found undigested, and nearly in the same state in all parts of the stomach, a circumstance which I was at first greatly at a loss to explain. This effect was uniform, I never saw it otherwise. Yet we must conceive that at the time the animal last eats, there is some food more or less digested in its stomach, and some gastric juice to act on part of that just received into it. The foregoing statements explain the difficulty. The division of the eighth pair of nerves destroys the secretion of the gastric juice ; but the animal still living, and the motions of the alimentary canal, as we have seen, being independent of the nervous influence *, the usual motions of the stomach continue, and send onwards into the intestines all the food which is digested, and consequently can apply to the stomach that stimulus which excites its natural

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of the division of these nerves in the neck, because they form various connexions with the great sympathetic nerve in the thorax. By dividing the eighth pair of nerves in the neck, the stomach is deprived of the whole, or nearly the whole, power of these nerves. It seems surprising that a warm-blooded animal should live long enough to afford proof of the functions of the stomach being perfectly performed after so severe and tedious an operation in the cavity of the thorax.

* See Chap. VI.

motions. Thus it is evident from the foregoing observations, that the undigested food must at length come into contact with it. As soon as this happens, the usual secretions not being supplied to produce the proper change in the food, an unnatural motion is excited; hence the efforts to vomit, which always ensue in about an hour, an hour and a half, or two hours, after the division of the nerves, marking the time when the stomach, having sent towards the pylorus its digested contents, begins to feel the effects of undigested food coming into contact with it. If the animal be allowed to eat after the operation, the efforts to vomit almost immediately ensue; the new food, as appears from the above statement, and indeed as is evident from the way in which it enters the stomach, directly coming in contact with some part of the small curvature. Thus we see the cause of the efforts to vomit, which follow the division of the eighth pair of nerves; and why, if the animal be allowed to live for a certain time after the operation, nothing but undigested food is found in the stomach. It also appears from the same circumstances, why the stomach is generally more distended than usual after the eighth pair of nerves have been divided, particularly if the animal has been allowed to eat after the operation; all the food, not digested by the gastric juice, present before the division of the nerves, remaining in the stomach.

SECT. II.

On the Effects on the Stomach and Lungs of destroying certain Portions of the Spinal Marrow, compared with those of dividing one or both of the eighth Pair of Nerves.

From the extreme irregularity of the motions of the alimentary canal, I have already had occasion to observe, we cannot ascertain whether it is subject to the influence of the different parts of the brain and spinal marrow in the way in which this has been done respecting the heart. I, therefore, endeavoured to ascertain this point by withdrawing from the most important part of this canal, the stomach, the influence of different parts of these organs, and observing the effects produced on it.

As we have seen the office of the stomach destroyed by the division of the eighth pair of nerves, we should at first view infer, that it is from the brain alone that the stomach derives its nervous influence. But although the process of digestion is suspended by the division of these nerves, it does not follow that the stomach may not derive nervous influence from other sources, because the loss of any considerable part of its nervous energy may destroy its function. Besides its remaining sensibility, indicated by the efforts to vomit, proves that its nervous influence is not wholly withdrawn by dividing the eighth pair of nerves.

If then this influence be not supplied to the stomach by the eighth pair of nerves alone, but also, as we have reason to believe from the evidence of anatomy, by nerves arising from different parts of the spinal marrow, it is evident, that cutting off its supply from any considerable part of this organ, while we leave the eighth pair entire, must affect its power, though probably not so much, because the brain we have reason to believe, constitutes the largest and most important part of the nervous system. To ascertain this point the following experiments were made.

Exp. 54. A hole was made about the middle of the spine, and the lower part of the spinal marrow destroyed by a small wire. The only apparent effect of the operation was the total paralysis of the lower part of the animal. It seemed to be otherwise in health. It may here be observed, that the sufferings of the animal in those experiments, in which portions of the spinal marrow were destroyed, was much less than at first view they appear to be; because as soon as any part of the spinal marrow is destroyed, the sensibility of all parts below it is lost. The animal, used in this experiment, was allowed to eat nothing for twelve or fourteen hours after the operation. At the end of this time it ate parsley very readily, and in large quantity, without any tendency to vomit. It lived twenty-four hours after the operation, and ate parsley from time to time.

On opening the abdomen after death, the stomach was found distended to a great degree, apparently containing the whole of the parsley which had been eaten after the operation, in an undigested state. It had passed no urine after the operation, and the bladder was so much distended, that it rose above the umbilicus. Some fæces had passed. The lungs collapsed on opening the thorax, but were slightly congested.

Exp. 55. In a full-grown rabbit a small wire was introduced into the spine at the fourth lumbar vertebra, by which it was attempted to destroy the spinal marrow as far as the first dorsal vertebra. The hind legs were rendered insensible and motionless. Respiration was a little disordered. In a short time after the operation the animal appeared lively and ate some parsley. The respiration continued to be slightly affected. Some hours after the operation Mr. Hastings, who watched the animal, observed it to be very cold, and it shivered, although it was kept in the same temperature with other rabbits, who shewed no signs of being cold. The rabbit used in the last experiment also seemed cold, but not in the same degree. The respiration now seemed much disordered, and the animal refused parsley. It was then brought near a fire and wrapped up in flannel. By these means it was soon relieved, the shivering ceased, its eyes looked more lively, and the breathing became more free. It was kept near the fire as long as it lived and frequently ate parsley. It died in twenty-seven hours after the operation.

On opening the abdomen we did not find the stomach much distended. The parsley near the cardiac orifice was not at all changed, and that near the pyloric orifice very slightly. The membrane of the trachea and bronchia was more vascular than natural. The bronchial cells were slightly loaded with frothy and bloody mucus, and there were the same red patches in the lungs as after dividing the eighth pair of nerves.

On examining the spinal marrow as far as the wire had passed we found blood extravasated in different parts, and its membranes were much inflamed. Immediately above the opening the spinal marrow was quite destroyed for about an inch in length. In other places it did not appear much injured.

Exp. 56. I wished to ascertain the effect of destroying a smaller portion of the spinal marrow than that destroyed in either of the last experiments, and requested Mr. Hastings to perform the following experiment, noting the temperature of the animal at the different periods of it. In a rabbit about two months old, fed on parsley, a small wire was introduced into the spinal canal, at the first lumbar vertebra, and that part of the spinal marrow which lies below this vertebra destroyed. After mentioning the other circumstances of the experiment, I shall throw together the observations made on the temperature. The animal lost the power of the lower extremities, but seemed in no other way immediately affected by the destruction of this part of the spinal marrow. It lived thirty-five hours.

On examining it after death, the stomach was found no larger than natural, the parsley retained its colour, smell and fibrous texture, although such a change had taken place in it, as demonstrated a very slight degree of the digestive process. The duodenum for about an inch below the pylorus was filled with parsley in the same state. The bladder and rectum were distended, but not so much as in the two last experiments. The lungs were slightly congested.

It is difficult to destroy a large portion of the spinal marrow without immediately killing the animal. It must be done very slowly, and even with this precaution the attempt will not always succeed. Its destruction is attended with no suffering to the animal, if we previously divide it at the upper part of the portion which we wish to destroy. On examining the lumbar portion of the spinal marrow, after death, it was found completely destroyed.

The following are the observations on the temperature.

The bulb of Farenheit's thermometer introduced into the mouth, and kept there for two minutes previous to the experiment, stood at 98°.

The animal was kept in a warmer temperature after than before the destruction of the lumbar portion of the spinal marrow. The temperature was always measured by putting the bulb of the thermometer into the mouth, and keeping it there for two minutes.

Immediately after the operation, therm.....	98°
In twelve minutes after it.......	92°
In half an hour after it	92°
In two hours and a half after it	98°
In five hours and three quarters after it....	98°
In seven hours and a quarter after it......	98°
In nine hours after it	96°
In ten hours after it	95°

The animal during all this time appeared lively and ate parsley.

In eleven hours after it, therm. ....96°
In twelve hours after it..................97°

Night coming on the temperature was not measured again for thirteen hours. In the morning the rabbit appeared lively and ate readily.

In twenty-five hours after the operation, therm.................................88°
In twenty-seven hours after it, therm.......84°
In twenty-nine hours after it ....88°
In thirty hours after it ....84°
In thirty-one hours after it ....84°
In thirty-three hours after it..............80°.

The animal still continued to eat. In thirty-four hours after the operation the temperature was 75°. In an hour after this, the animal died. This animal did not appear nearly so cold as that in the preceding experiment, in which a larger and more important part of the spinal marrow was destroyed.

Thus we find the function of the stomach impeded by depriving it of the influence of any considerable part of the spinal marrow, and it seems only more affected by the division of the eighth pair of nerves, in proportion to the greater extent and importance of the brain.

It is remarkable, that the result of the first of these experiments is the same with that which M. le Gallois obtained when he had divided one of the eighth pair of nerves in a guinea-pig. The animal did not vomit, and the stomach was found distended to a great size, apparently containing all the food it had taken after the operation in an undigested state. This coincidence demonstrates how much the same the effect on the stomach is, whether we deprive it of part of the nervous influence, which it receives from the brain, or part of that which it receives from the spinal marrow. Mr. Hastings, at my request, made this experiment on a rabbit.

Exp. 57. One of the eighth pair of nerves was divided in a rabbit. No difficulty of breathing immediately ensued. The rabbit continued to eat from time to time, with occasional attempts to vomit, and once it brought up a little of the parsley. In the course of the experiment, it laboured under a slight degree of dyspnœa. A short time before its death, which happened in twenty-four hours and a half after the division of the nerve, the dyspnœa suddenly increased with restlessness.

On examining the stomach after death, we did not find it much distended. The food was very little changed. The œsophagus did not contain much food in the upper, in the lower part it was much distended with it. The bronchia were much less loaded than when both nerves had been divided. The larynx was found quite full of the parsley, in consequence of the epiglottis's having, by some strange accident, been caught in the membrane of the pharynx, so as to prevent its falling down on the glottis. In consequence of this accident, which was evidently the cause of death, the experiment was repeated.

Exp. 58. One of the eighth pair of nerves was divided in a rabbit. It ate soon after the operation, but did not attempt to vomit till two hours and a half after it, and then dyspnœa came on. The breathing at times was almost free, and the efforts to vomit only occurred at intervals. Both subsided when it was prevented from eating. It died in forty-five hours after the operation.

The stomach was found after death larger than natural, being distended with flatus, and containing more food than usual. For the most part the parsley was in the same state as when taken into the stomach, both in appearance and smell. In some places it was slightly changed. There was undigested parsley in the duodenum, to the distance of about an inch from the stomach. The lower end of the œsophagus contained a little parsley. There was none in any other part of it.

The membrane of the trachea was of a

darker colour than natural, its vessels being distended with blood, and there was some frothy mucus in it. The lungs were slightly congested. The membrane of the bronchia was too vascular, and the air-cells contained some frothy mucus. All these appearances existed in a much less degree than when both nerves were divided. The lungs collapsed imperfectly on opening the chest. There were some dark coloured spots on them.

If the reader will take the trouble to compare these appearances with those observed when part of the spinal marrow was destroyed, particularly in Experiment 55, he will see that the division of one of the eighth pair of nerves produces nearly the same effect on the lungs and stomach, as the destruction of part of the spinal marrow.

I wished to see the effect on the stomach and lungs of destroying nearly the whole spinal marrow. But with all the precautions that could be taken the animal died almost immediately. It is difficult indeed to prevent immediate death, when as much of it is destroyed as in Experiments 54 and 55.

There is still another point in this part of the subject, which remains to be ascertained. Do the effects observed in the stomach and lungs, when part of the spinal marrow is destroyed, arise from the destruction of that part; that is, from the ceasing of its office, or from the influence of the brain on the spinal marrow being thus limited? It is evident, that if the former opinion be correct, the division of the spinal marrow in the middle, will not produce the same effects as the destruction of the lower half. If the other opinion be correct, these must produce precisely the same effects.

Exp. 59. The spine was divided in an old rabbit, about the same place at which it was opened in order to destroy the lower half of the spinal marrow in Experiment 54, after which there was no motion in the lower extremities. The rabbit seemed lively after the operation, and continued to eat frequently till within six hours of its death. It died in twenty-seven hours and a half after the division of the spinal marrow. It had not vomited, and had had little or no dyspnœa.

On examining the stomach after death, it was not found more distended than natural. The food it contained, was nearly as well digested as in the stomach of a healthy rabbit. The contents of the duodenum had completely undergone the proper change. The bladder and rectum were distended, but not so much as after the destruction of the lower part of the spinal marrow.

The lungs collapsed on openng the thorax but contained a little frothy mucus.

On examining the spine, it was found to have been completely divided.

On comparing this experiment with Experiment 54, we see that here the lower part of the spinal marrow still performed its office, and supplied its portion of nervous influence to the stomach and lungs, although the communication between it and the brain was cut off.

The reader must have perceived through the whole of these experiments, that any considerable diminution of the nervous influence almost wholly deprives the stomach of its power; and even the slightest diminution of it seems to be felt. I have no doubt, that we may ascribe the very slight derangements observed in the stomach and lungs in the last experiment, to the destruction of function, that must have taken place in the part of the spinal marrow at which it was divided. The bruise occasioned by the wound must of course have destroyed the function of a small part on each side.

Thus we find that although we cannot by agents, applied to different parts of the brain and spinal marrow, ascertain how far the stomach and lungs are under their influence, we may, by withdrawing the influence of different parts of the former organs, prove that the stomach and lungs, like the heart, are capable of being influenced through every part of them.

As the functions of the stomach and lungs depend on the secreting power, the same inference, it is evident, applies to it; and consequently, as will appear more fully from what is said in the ninth chapter, to the vessels of secretion.

CHAP. VIII.

On the Cause of Animal Temperature.

WE are now to attend to the temperature of the animals in those experiments, in which portions of the spinal marrow were destroyed. It appears from them that while the destruction of part of the spinal marrow impedes the office of secreting surfaces, it also lessens the evolution of caloric. Mr. Brodie, in the Croonian Lecture for 1810, gave an account of experiments which led to the inference, that the production of animal temperature is under the influence of the nervous system: and in the Philosophical Transactions of 1812, he relates additional experiments, tending to strengthen this inference. In the second section of the last chapter, I have had occasion to relate experiments made for other purposes, which tend in a striking manner to confirm the opinion of Mr. Brodie. He found that poisons, impairing the vigour of the nervous system, impair the temperature. In the foregoing experiments, lessening the extent of this system by destroying part of the spinal marrow, had the same effect.

Towards the conclusion of the latter of the above papers, Mr. Brodie observes, " The " facts here, as well as those formerly ad" duced, go far towards proving, that the tem" perature of warm-blooded animals is con" siderably under the influence of the nervous " system; but what is the nature of the con" nexion between them? Whether is the brain " directly or indirectly necessary to the pro" duction of heat? These are questions to " which no answers can be given, except such " as are purely hypothetical. At present " we must be content with the knowledge of " the insulated fact: future observations may, " perhaps, enable us to refer it to some more " general principle."

The various phenomena of animal temperature, and the experiments on this subject, related in the last chapter, compared with the effects on secreting surfaces, observed in these experiments, seem to me to prove, that the caloric, which supports animal temperature, is evolved by the same means, namely, the action of the nervous influence on the blood, by which the formation of the secreted fluids is effected, and consequently that it is to be regarded as a secretion.

I shall relate some additional experiments on animal temperature, both when treating of the order in which the functions cease in dying, and in the observations which I shall have occasion to make on the nature of the vital powers, tending to confirm this view of the subject.

CHAP. IX.

On the Use of the Ganglions.

It appears from experiments related in the first and second chapters of the present part of this Treatise, that the motion of the heart, though independent of, may be influenced through, every part of the brain and spinal marrow. It seems also ascertained by experiments related in the same chapters, that the blood-vessels bear the same relation to the nervous system with the heart. Their power is equally independent of this system, and they are influenced in the same way by agents acting through it. We cannot, we have seen, affect the muscles of voluntary motion in the extremities by agents applied to the upper parts of the brain, yet the vessels of their most extreme parts obey agents applied even to the upper surface of this organ.

It appears from experiments related in Chapter VI., that the muscular power of the alimentary canal is also independent of the nervous system. It is impossible, for reasons which have been laid before the reader, to ascertain by experiments similar to those relating to the heart and blood-vessels, whether the alimentary canal is subject to the influence of every part of the brain and spinal marrow. With respect to it, therefore, I attempted, in the second Section of Chapter VII., to determine this point in a different way. Although the muscular power of the alimentary canal is independent of the brain and spinal marrow, its secreting power, we have found, is wholly dependent on them; I endeavoured, therefore, to ascertain whether it is subject to every part of these organs, by withdrawing the influence of different parts of them from the most important part of it, the stomach; and we have seen that when this organ is deprived of the influence of any considerable part either of the brain or spinal marrow, its secreting function is deranged, the derangement being proportioned to the importance and extent of the part whose influence has been withdrawn. The stomach, therefore, like the heart, is capable of being influenced by every part of the brain and spinal marrow. The same observation, we have seen, applies to the lungs.

Here the question arises, By what means is the one set of organs subjected to the influence of every part of the other? We cannot suppose that the former receive nerves from every part of the brain and spinal marrow. We know, indeed, that no organ does so. The following seems to be the state of the question. We see some parts influenced by every part of the brain and spinal marrow, others only by small parts of them. In the latter instances, we see directly proceeding from those small parts, the nerves of the parts influenced. In the former instances, namely, where it is found that the part is influenced by all parts of the brain and spinal marrow, we do not in any case see nerves going directly from all parts of these organs to the part influenced, but we always see this part receiving nerves from a chain of ganglions, to which nerves from all parts of the brain and spinal marrow are sent. It is, therefore, evident from direct experiments, that the nerves issuing from ganglions convey to the parts, to which they send nerves, the influence of all the nerves which terminate in these bodies.

Such then is the relation which the most important organs of involuntary motion bear to the brain and spinal marrow. Their powers are independent of both, yet they are subjected to the influence of every part of both, communicated through the medium of the ganglions; and when we see the other organs of involuntary motion equally independent of the brain and spinal marrow, and supplied with nerves from ganglions in the same way with them, it is allowable to infer that they bear the same relation to the brain and spinal marrow. Thus it would appear, that the ganglions may be regarded as a secondary centre of nervous influence, receiving supplies from all parts of the brain and spinal marrow, and conveying to certain organs the influence of all those parts.

If the nervous influence of the thoracic and abdominal viscera be thus supplied from a common source, why, in affections of the spinal marrow, it may be said, is the breathing most affected when the disease is in the dorsal portion of this organ, and the action of the bladder and rectum most affected when its chief seat is in the lumbar portion? This arises from the muscles of respiration deriving their nerves from the dorsal portion, and the abdominal muscles deriving their nerves from the lumbar portion of the spinal marrow. The latter muscles generally excite, or at least increase, the action of the bladder and rectum, by pressing them against their contents, and also by this pressure contribute mechanically to expel their contents. Thus, in the above cases, in addition to the failure of nervous influence in the viscera, there is a failure of excitement in the muscles of voluntary motion, which conspire with these viscera in certain parts of their functions *.

We can trace the communications of nerves issuing from the great chain of ganglions, placed, it would seem, to facilitate these communications in the centre of the animal system, with all the nerves of the body. And many circumstances, regarded by anatomists as anomalous, namely, nerves becoming larger after they appear to send off branches, apparently taking a retrograde course, &c., are readily explained, if we admit that nerves, arising from ganglions, join and again separate from those proceeding in an opposite direction from the brain and spinal marrow. It is worthy of remark that none of these anomalous appearances are observed in the extremities, where the ganglian, must take the same course with the other, nerves. Bichat, although his opinions respecting the use of the ganglions are

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* See what is said of the muscles of inspiration in Chapter X.

very different from those which I have been led to form, and indeed wholly inconsistent with the results of the foregoing experiments, was induced from his observation of the situation and distribution of the ganglions and their nerves, to regard them as the centres of nervous systems.

Comparing all that has been said, we have reason to believe, that the system of ganglian nerves is quite as extensive as that of the nerves proceeding directly from the brain and spinal marrow. We every where find blood-vessels, which we have seen receive the nervous influence through the ganglions; and, indeed, in the larger vessels, we can often trace the ganglian nerves attached to and supplying them. The following case, related by Dr. Parry, in the 139th page of his Treatise on the Arterial Pulse, might alone be regarded as proving the existence of two sets of nerves in the extremities; the one supplying the muscles of voluntary motion, the other the powers supporting the circulation; and strikingly illustrates what has been said on this subject. " I have seen," he observes, " a total loss of pulse in one arm with " coldness, but complete power of motion in " that part; while the other arm was warm, and " possessed a perfectly good pulse, but had lost " all power of voluntary motion."

From the foregoing observations the question arises, For what purpose has nature thus combined the influence of every part of the brain and spinal marrow to bestow it on particular parts? This question appears to be answered by the experiments which shew, that when the influence of any considerable part, either of the brain or spinal marrow, is withdrawn from secreting surfaces, the secreting power is deranged. This we have seen ascertained by repeated experiments, both with respect to the surface of the stomach and lungs. Among the secretions I ranked the evolution of caloric, although not taking place on any particular surface, because it appeared to be performed by the same power acting on the same fluid; and because, like secreted fluids, it fails when any considerable part of the influence of the brain or spinal marrow is withdrawn.

Admitting, it may be said, that the due performance of secretion requires the united power of all parts of the brain and spinal marrow, and that we may, therefore, explain why their united influence is bestowed on secreting surfaces; the question still remains, Why should their united influence be bestowed also on the muscles of involuntary motion?

It is evident that affections of the nervous system could produce no occasional increase of the secretions, were not the sanguiferous system, and particularly the vessels of secretion, capable of being stimulated by the same influence which operates in the formation of the secreted fluids. The increase of secreting power in any part would be in vain, were there not at the same time a corresponding increase in the supply of the fluids on which it operates. A similar observation applies to the excretory muscles as far as they are muscles of involuntary motion. The same increase of nervous influence which occasions an increased flow of secreted fluids, excites these muscles to carry off the increased quantity. Nature does not seem to trust this to the increase of stimulus occasioned by the increased flow of the secreted fluid, which we have reason to believe from the *modus operandi* of certain causes of inflammation, would often occasion morbid distension *. Is it not more than probable that the same laws obtain in the absorbent system? Now, the vascular system and the muscles of excretion, if in them we include the alimentary canal, comprehend all the muscles which are supplied with nerves from the ganglions, unless we regard the iris as a muscle. The state of this organ is quite anomalous in the animal economy, being one of involuntary motion, always stimulated through the medium of the nervous system.

Thus, we see the necessity of every part of the function which the ganglions appear to perform. A combination of the whole nervous influence is necessary for the due formation of the secreted fluids, and that there may be, under all circumstances, both a due supply of the fluids to be acted upon, and a due removal of those prepared, whether for the functions of life or for the purpose of being thrown out of the system, it is necessary, as appears from what has just been said, that the powers which convey these fluids should be subjected to the influence by which secretion is performed *. This function, it is evident, requires a more regular supply of fluids than could have been obtained, had the usual action of the vessels depended on the nervous system, which is subject to continual variation; but had not this system been capable of influencing the vessels, no change in it could have influenced the flow of secreted fluids. Thus, it is necessary that the power of the sanguiferous should be independent of the nervous system, yet capable of being influenced by it; as it is ascertained to be by the experiments related in the first and second chapters of the present part of this Treatise.

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* See Part III., Chap. II.

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* The constant presence of fluids in secreting surfaces appears to solicit a continual supply of nervous influence to them, so that they go on during our sleeping as well as waking hours. The more copious the supply of fluids to secreting surfaces, we find the secreting power the greater, and *vice versâ*.

That the reader may see how far the observations of the anatomist correspond with the result of the preceding experiments, I shall beg leave to recall to his mind the nervous connexions of the ganglions; by which he will find that they may receive the influence of every part of the brain and spinal marrow, and communicate that influence to every part of the body. The great sympathetic nerve receives nerves from every part of the spinal marrow, being largest near the middle of the spine, and becoming smaller as it ascends and descends, forming ganglions, which give out nerves on all sides. When these circumstances are compared with the fact of its conveying the influence of every part of the spinal marrow, we cannot, I think, hesitate to regard it as arising from this organ; especially as its slender communications with the nerves of the head present the appearance of its gradual termination in that direction. This inference is farther strengthened by other means being provided for conveying the influence of the brain to the thoracic and abdominal viscera. The *par vagum* of the eighth pair of nerves, we have seen from the effects of dividing it, performs this office; for which it is admirably fitted, by its numerous and extensive communications with the ganglions and plexuses of the great sympathetic. After various connexions with those in the neck and chest, it sends a large branch to the stomach, whose filaments are intermixed on this organ with those of nerves sent by several of the abdominal plexuses; and at length terminates in forming with the splanchnic branches of the sympathetic the great semi-lunar ganglion, called the cœliac ganglions from its situation, and its being composed of many small irregularly-shaped ganglions of different sizes. From these ganglions, so formed, many nerves issue, forming plexuses on the different large arteries, from which they derive their names. Nerves from these plexuses alone, or intermixed with other branches from the sympathetic nerves, supply the whole abdominal viscera.

Before the sympathetic nerves finish their course by uniting on the os coccygis, they form ganglions in the loins, which send branches to the lumbar nerves; and others in the pelvis, which send branches to the sacral nerves; thus forming communications between the ganglian system and the nerves of the lower extremities.

The nerves of the upper extremities communicate with this system, both by means of the middle cervical ganglion, and through the sympathetic nerves, by branches from the second and third intercostal nerves which go to these extremities.

The ganglian system communicates by branches of the sympathetic nerves with the internal nerves of the head. One branch is sent to the second branch of the fifth pair before it leaves the cranium, and one, two, or sometimes three small filaments to the sixth pair, and a branch to the *portio dura* of the seventh pair, at the under part of the ear. These connecting branches are generally regarded as proceeding from the nerves of the head, but for reasons already assigned, we must, as far as I am capable of judging, agree with those writers who regard them as proceeding from the sympathetic nerves. The extensive connexions of these nerves with the eighth pair have already been mentioned.

The external nerves of the head and neck, namely, the higher spinal nerves, communicate with the cervical ganglions; and lastly, the ganglian system communicates with the external parts of the trunk by means of the connexions of the sympathetic with the spinal nerves which supply those parts*.

Thus the sympathetic nerve, conveying the influence of the spinal marrow, and the *par*

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* Here we have reason to believe a double communication takes place, the spinal nerves conveying to the sympathetic the influence of the spinal marrow, and the sympathetic sending with them to the parts to which they are distributed, filaments conveying the influence of the ganglian system.

vagum, that of the brain, unite in forming the ganglions, which with their plexuses, constitute a secondary centre of nervous influence, a channel through which the influence of every part of the brain and spinal marrow flows, to be bestowed on the thoracic and abdominal viscera, on the vessels and all secreting surfaces; the most important of which parts, we have by direct experiment found subjected to every part of the brain and spinal marrow.

In one of the Treatises referred to in the Report of the Institute of France, that by Dr. Johnstone, the reader will find many facts respecting the ganglions and the distribution of their nerves, which he collected with much assiduity for the purpose of supporting his opinions respecting the uses of these organs. He adopted the opinion of Winslow and other physiologists, that the " ganglions seem analogous to the brain in their office; subordinate springs and reservoirs of nervous power," he continues, " they seem capable of dispensing it, long after all communication with the brain is cut off. And, although they ultimately depend on the brain for its emanations, it appears from facts, that dependence is far from being immediate and instantaneous*." The reader will readily perceive that this opinion is incompatible with many of the facts which have been laid before him. Dr. Johnstone was led to infer, that the ganglions can, for a certain time, perform the office of the brain, by ascribing to the power of nervous influence many phenomena which seem wholly to depend on the power of the muscular fibre itself*.

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* Med. Essays and Obs. by J. Johnstone, M. D., Physician in Worcester, 1795, p. 85.

To the above opinion, adopted from his predecessors, Dr. Johnstone added the following opinion of his own, which he endeavours to support by direct experiment, as well as by an accurate and extensive review of the phenomena of the nervous system. "May we not "reasonably conclude," he observes, "that "ganglions are the instruments by which the "motions of the heart and intestines are from "the earliest to the latest periods of animal "life rendered uniformly involuntary †?"

Dr. Johnstone's experiments, an account of which is given in the 25th and following pages of the work just quoted, and other experiments of a similar nature to which he refers, of Haller, Whytt, &c., were made with a view to prove that it is impossible to affect the action of the heart by stimuli applied to the brain and spinal marrow. These physiologists appear to have been deceived in the result of their experiments on this subject by two circumstances. They did not employ the precaution of preventing the action of the muscles of voluntary motion, which renders it impossible to judge of the effect of the stimulus on the heart: and they were not aware that the heart will not obey a stimulus applied to the brain and spinal marrow, however powerful, unless it be applied to a portion of considerable extent. Any person who attends to these precautions will find, that the heart is not only as easily stimulated through the brain and spinal marrow as the muscles of voluntary motion are; but that it may be stimulated through them for a considerable time after these muscles can no longer be influenced in this way; proving that the ganglions oppose no obstacle to the influence of the brain and spinal marrow being extended to the muscles of involuntary motion.

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* Chap. VI.

† The above-mentioned Treatise, p. 16.

We can surely be at no loss to account for the action of these muscles being involuntary, when we know that they are all exposed to the constant or constantly renewed action of stimuli, over which the will has no power. Besides, the action of these muscles produces no sensible effect. We will to move a limb, not to excite a muscle. We wish to handle, for example; and, on trial, find that we can move our fingers; but what act of volition could we perform through the medium of the heart or blood-vessels? If we had no wish to handle, the muscles of the fingers of course would never become subject to the will. Few have any command over those of the external ear; and it deserves to be remarked, that the will influences the rectum and bladder, the only internal organs which can assist in accomplishing an end desired.

It seems to be superfluous, after the experiments which have been related, to say any thing in refutation of the opinion of Bichat, that the ganglions are centres of nervous influence, independent of the brain and spinal marrow. "Les nerfs des ganglions ne peuvent trans-"mettre l'action cerebrale; car nous avons "vu que le système nerveux partant de ces "corps, doit etre considéré comme parfaite-"ment independant du système nerveux céré-"bral; que le grand sympathique ne tire point "son origine du cerveau, de la moelle épinière "ou des nerfs de la vie animale; que cette "origine est exclusivement dans les gang-"lions; que ce nerf n'existe meme point, à "proprement parler, qu'il n'est qu'un en-"semble d'autant de petits systèmes nerveux "qu'il y a de ganglions, lesquels sont des "centres particuliers de la vie organique, ana-

" logues au grand et unique centre nerveux de " la vie animale, qui est le cerveau."—*Recherches Physiologiques sur la vie et la mort, par Xav. Bichat,* page 355 & seq.

CHAP. X.

On the Relation which the Vital Powers bear to each other, and the Order in which they cease in Dying.

It is evident that before we can attempt to trace the relation which the vital powers bear to each other, and the order in which they cease in dying, we must be able clearly to define them.

It appears from the experiments which I have had occasion to relate or refer to, that in the more perfect animals there are three vital powers not directly depending on each other, the sensorial, the nervous, and the muscular powers.

With regard to the last of these, it is readily distinguished by its functions, which cannot be confounded with those of either of the other powers, and has its seat, we have seen, in the muscular fibre itself.

The nervous power, it appears from many of the experiments which have been related, acts only as a stimulus to the muscular fibre. It performs the more complicated functions of preparing the various secreted fluids*, and causing an evolution of caloric from the blood; and is the means by which impressions are conveyed to and from the sensorium.

The sensorial power, as far as it is concerned in the functions of mere animal life, appears to consist wholly in receiving impressions from, and communicating them to, the nervous power.

The seat of the sensorial and nervous powers is not so well defined as that of the muscular power. M. le Gallois appears to regard the brain as the seat of the one, and the spinal marrow as that of the other; but many observations seem to oppose this opinion. Nerves proceeding wholly from the brain exhibit all the phenomena of nervous power properly so called, and that the spinal marrow possesses sensorial power appears from very simple experiments.

Exp. 60. The spinal marrow of a rabbit was divided about the middle. When one of the hind legs was wounded, not only the wounded leg was moved, but the other hind leg also, demonstrating that there is a power residing in the spinal marrow which receives the impression made on the nerves of the one leg and communicates it to those of the other. M. le Gallois makes many similar observations. In the cold-blooded animal the same thing is observed in a greater degree. For some hours after decapitation the frog will often sit in its usual position, and appear sensible to an injury inflicted on any part of it. It is evident from many observations, however, that the sensorial power chiefly resides in the brain, and the nervous in the spinal marrow.

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* As the secreting power not only prepares, but preserves for their proper uses, the secreted fluids; it must regulate the due absorption as well as secretion of these fluids, and therefore the whole process of assimilation.

If these powers, it may be said, are thus blended in their organs, what proof have we of their being distinct powers? This proof, I think, we shall find by carefully observing the process of dying, of which what we call death appears to be only the first stage. We shall also, I think, by the same means, clearly perceive the way in which all the foregoing powers are so connected in the more perfect animals, that none can long exist without the others.

At the instant of death, it is evident, the sensorial power ceases. No impression made on any part of the body is perceived or followed by any act of volition. It is equally evident to the physiologist that the muscular power still remains. If the heart or muscles of voluntary motion be stimulated, they still possess the power of contraction which is only lost by slow degrees a considerable time after the sensorial power has ceased to exist. It is also evident to the physiologist that some part of the nervous power still exists, for if the nerves themselves, or those parts of the brain or spinal marrow from which they originate, be irritated, the corresponding muscles are thrown into action; a proof that the extinction of the sensorial powers does not depend on the nerves having become incapable of conveying impressions. Is the nervous power still capable of performing its other functions? Whether it is capable of conveying impressions to the sensorium we have of course no means of judging, where no sensorium exists; but if we find it capable of all its other functions, we may infer that it is capable of this function also, and that the cause of its non-appearance is the known failure of the power which must conspire to make it sensible. The question then which we have to consider here is, can the nervous power effect the formation of the secreted fluids, and occasion an evolution of caloric from the blood, as we find it can excite the muscles, after the destruction of the sensorial power?

I have already had occasion to refer to Mr. Hunter's observations respecting the digestion of the stomach after death. It is perhaps superfluous to observe that this is not to be regarded

as any vital action. It is a mere chemical process. But Mr. Hunter, as appears from the following observations, suspected that a truly vital action continues in the stomach for some time after what is called death. " This is ex-
" actly the case with the experiments of Spal-
" lanzani, which although they prove that meat
" was digested in the stomach after the animal
" was killed, which no one doubted," that is, no one doubted that the gastric juice already in the stomach would continue to perform its office there, " yet are not at all calculated to shew that
" the stomach itself may be digested. In fact,
" the manner in which they were managed
" rather tended to prevent that effect from
" taking place, the gastric juice having sub-
" stances introduced on which it could act,
" was less likely to affect the coats of the sto-
" mach *. That the digestion was not carried
" on merely by the effects of the gastric juice
" secreted before death is evident from his own
" account, some of the food which had been
" introduced and digested being found in the
" duodenum ; a thing which could not have
" happened if a cessation of the actions of life
" in the involuntary parts had taken place

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* This observation, as appears from what is said above, is incorrect. It is soon after a full meal that the stomach is most apt to be digested after death, the cause of which I have had occasion to explain. See Chap. VII. Sect. I.

" when visible life terminated. There had
" been an action *and most probably a secretion*
" *in the stomach* *."

It appeared to me that this conjecture of Mr. Hunter might be reduced to the test of experiment, by dividing immediately after death the eighth pair of nerves, which seems at once to destroy the secretion of gastric juice. I shall use the words death and kill in the usual acceptation, not implying the ceasing of all the functions. After this explanation no ambiguity can arise from the use of these terms. We are not, it is evident, to expect that any great secretion of gastric juice can take place after death, or consequently that any great difference can be observed between the food in the stomach of an animal in which the eighth pair of nerves has been divided immediately after death, and one in which they are left entire, and many circumstances which we cannot estimate, particularly there being more gastric juice in the stomach of the one animal than the other at the time they are killed, or one having eaten more than the other, must influence the result. It will not answer the purpose, it is evident, to confine the animals to the same quantity of food, because the stomach of that which is most hungry will digest it most quickly. The quantity of old food in the stomach also influences the result. The question, therefore, can only be determined by making the experiment on a large scale, to which, as it is made on the dead animal, there can be no objection.

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* Observations on the Animal Œconomy, page 181.

Exp. 61. This experiment was made on twenty-six rabbits; eight full grown, eight half grown, six two months old, and four one month old. They were made to fast for sixteen hours, at the end of this time allowed to eat as much cabbage as they chose, and then killed by a blow on the occiput. Immediately after death the eighth pair of nerves was divided in one half of those of each description, and they were all allowed to lie undisturbed for about twenty-two hours. I need hardly observe that the experiment was not made on all at the same time, but care was taken that the circumstances of it should be the same in all. The stomachs were then laid open, and those of the rabbits of the same age, who had eaten most nearly the same quantity, were compared together. The result was, that in twelve pairs the food was most digested in those animals whose nerves were left entire. In one pair it was most digested in the animal whose nerves had been divided. In several of those whose nerves had been divided, the cabbage appeared quite fresh and green. This did not happen in any whose nerves were left entire. In these the colour was always changed more or less to a brown. The difference in the state of the cabbage was sometimes more sensible to the touch than to the eye, that least digested feeling hardest. This experiment, at the same time that it proves the continuance of secretion after the sensorial power has ceased to exist, shews more than any experiment made on the living animal could do, how quickly the secretion of gastric juice is destroyed by the division of the eighth pair of nerves in the neck.

It is remarkable that the division of these nerves after death almost always produced the same appearance of dark-coloured patches upon the surface of the lungs, but generally in a less degree, observed from it when the operation had been performed during the life of the animal; an effect, equally with the state of the stomach, demonstrating that some of the involuntary functions continue for a certain time after visible death. These patches now and then appear in the lungs of an animal whose nerves are entire, after it has lain dead for some time; but much less frequently, and to a much less degree, than when the nerves have been divided immediately after death.

The appearance of dark-red patches on the surface of the lungs, we have seen, is always observed to a great degree when the eighth pair of nerves have been divided during the life of the animal, and it has survived the operation many hours. It may be regarded, and is mentioned by various physiologists, as the characteristic effect of the operation on this organ. The congestion of the lungs, which is also an uniform consequence of it, appears under many other circumstances, but I know of no other in which there is an appearance like this patching, except, as I have just mentioned, that a certain degree of it, or rather something like it, now and then appears in the lungs of the entire animal after it has lain dead for many hours. In the living animal it was always proportioned to the degree in which the secreting power of the lungs was deranged, appearing to the greatest degree when the congestion of the lungs was greatest.

In considering the result of the foregoing experiment, the question arises, What occasions any supply of fluids to secreting surfaces, after the circulation has ceased, and thus enables the remaining nervous influence to produce any secreted fluid after death? The result of the following experiments appears to afford a ready answer to this question.

Exp. 62. A rabbit, about two months old, was killed by a blow on the occiput. The chest was then laid open and a ligature thrown round the aorta. Part of the mesentery was now brought before a microscope, and the blood in its vessels seen both by Mr. Sheppard and myself, moving with great velocity. By examining different parts of it, and choosing those which had not been previously disturbed, and consequently still retained some warmth, we found the circulation, in the smaller vessels, going on with rapidity for a quarter of an hour after the aorta had been secured, and an irregular motion of the blood in these vessels was evident for twenty minutes longer, the blood stopping and going on, and sometimes moving backwards and forwards in the same vessel. This could be distinctly seen long after the part had become quite cold. This experiment was performed in the sun-shine, in the open air, where there happened to be a good deal of wind, and the exposed part of the mesentery quickly became parched; which, as we found from other trials, destroyed the motion of the blood in the capillaries long before it naturally ceases.

Full-grown rabbits are bad subjects for this experiment, on account of an accumulation of fat which takes place in the mesentery and obscures the vessels. Rabbits about six weeks old, when they have been fed for some time on green meat, are generally thin, and consequently the best subjects for it.

Exp. 63. A dead rabbit, about a month old, whose intestines we had been examining, after having thrown a ligature round all the vessels attached to the heart and removed this organ, was thrown aside, with the intestines hanging out through the wound in the parietes of the abdomen. An hour and a quarter after the heart had been removed, I brought part of the mesentery, which had long been quite cold, before the microscope, and still found the blood in some of the capillary vessels moving freely. I have no doubt that the blood continues to move in the capillaries of a full-grown rabbit, whose temperature will sink much more slowly, for several hours after death. This at the same time accounts for the supply of fluids to secreting surfaces, and for a certain power of the nervous system remaining after death, and when the *vis à tergo* has wholly ceased, except as far as it depends on mere elasticity and the action of very small vessels. It seems to be owing to this cause also that the larger arteries of dead animals are found empty *. How readily the continued action of the capillaries must empty them, will be evident when we recollect how much the sum of the areas of the branches of the arteries exceeds the areas of their trunks.

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* I cannot agree with Dr. Parry in ascribing this fact to the contractility of the arteries. This may reduce, but it cannot wholly expel, their contents.

We are now to inquire whether the nervous power is capable of occasioning an evolution of caloric from the blood after the sensorial power has ceased to exist. I here consider it as proved by experiments already laid before the reader, or referred to, that the evolution of caloric is a function of the nervous influence. But it seems so immediately to depend on the existence of the circulation, and is so generally proportioned to its vigour, that we cannot, I think, adopt a better means of answering the question before us, than by ascertaining whether supporting circulation by artificial respiration after death occasions a greater evolution of caloric than takes place when the dead animal is left undisturbed. On this subject there has been great difference of opinion. The following experiments seem to point out how this difference may have arisen, on the supposition that all the experiments which have been made on the subject are correct, which we have every reason to believe them to be.

Exp. 64. Two rabbits of the same size were killed by a blow on the occiput, the temperature of the air being 61°, that of both rabbits 104°. The lungs of one were inflated six times, those of the other from twenty-six to thirty times, in a minute. The temperature of the first in half an hour was 102.25°, in an hour 100°; the temperature of the second at the end of half an hour was 101 5°, at the end of an hour 98°.

It is evident that all the air thrown into the lungs, beyond what is necessary to effect the proper change in the blood, must tend to reduce the temperature in proportion as that of the air is less than that of the animal. The living animal receives but little air into the lungs in one inspiration. It is impossible in the dead animal to throw in the quantity which the blood still demands, and no more.

The following experiments, in which Mr. Sheppard was so good as to assist me, as indeed he did in all the experiments which I made on this part of the subject, strikingly illustrate these observations.

Exp. 65. Two rabbits were chosen of the same size, and each of the temperature of 102 5°. They were killed in the usual way, in the temperature of 65°; one was left undisturbed. In the other, the lungs were inflated about thirty times in a minute. In half an hour the temperature of the undisturbed rabbit was 98.75°, while that of the other was only 98 5°. In the last the lungs were then inflated only about twelve times in a minute. In half an hour its temperature was 96°, so that it had lost 2.5°, while that of the other left undisturbed had in the same time sunk to 95.25°, so that it had lost 3.5°.

Exp. 66. Two rabbits were killed in a temperature of 61.5°. The temperature of the one was 106°, of the other 103°. The lungs of the first were inflated twelve times in the minute, the other was left undisturbed. In half an hour the first had lost 3.5°, its temperature being 102.5°. The other in the same time had lost 4°, its temperature being 99°. The first being of the highest temperature, would have cooled fastest had both been undisturbed, although probably not in a sensible degree. I may here observe, that it happened in the course of such experiments as those which I am relating, that the temperature of the room varied, but as the experiment was always made on both rabbits at the same time, and placed together, this could not influence the result, and is therefore unnoticed. The lungs of the first of the above rabbits were now inflated at the rate of from twenty-six to thirty times in a minute. At the end of half an hour its temperature was 98°, that of the other at the same time being 94.5°, so that each had now cooled 4.5°, the evolution of caloric in consequence of the inflation of the lungs being here sufficient to counteract the cooling effect of the rapid change of air and no more. In one experiment of this kind, in which the lungs were inflated only a few times in a minute, we found that the temperature had risen nearly 1° between two of the examinations.

While I was making experiments on this subject in Worcester, Mr. Hastings was, without my knowledge, making similar experiments at Edinburgh. He shewed me the detail of several, which prove that throwing air into the lungs of the dead rabbit about fifteen times in a minute, occasions it to cool more slowly than it would otherwise do. In one of his experiments the rabbit, in which artificial breathing was performed, cooled only 4°, while that which was left undisturbed cooled 7.5°. This was the greatest difference he observed. He frequently saw the thermometer rise a little in those animals in which the lungs were inflated after death. In those in which they were not inflated the cooling was always uniform.

There can be no doubt, I think, from the preceding experiments, that when the lungs are not inflated so frequently as to constitute a powerfully cooling process, their inflation by occasioning an evolution of caloric after what we call death, retards the cooling of the animal.

My next object was to ascertain how far the evolution of caloric, after death, is influenced by the destruction of the brain and spinal marrow.

Exp. 67. Two rabbits of the same size, whose temperature was 98°, were killed in the usual way. In one, immediately after death, the brain and spinal marrow were destroyed by introducing, through a hole in the cranium, a wire of nearly the same diameter with the cavity of the spine, repeatedly pushing it on to the end of this cavity, and then moving it about for some time in the cavity of the cranium. The other rabbit was left entire. A hole was made about the centre of the abdominal muscles in each, to admit of a thermometer being introduced into the cavity of the abdomen. They were placed near each other in a temperature of 50°. During the first twenty minutes each lost exactly 4°, and they both lost, during the succeeding three quarters of an hour, just 2° during each quarter. Something, which we could not ascertain, accelerated the rate of cooling during the next quarter, and so exactly did it correspond in both rabbits, that each lost during this quarter 2.5°. After this their temperature diminished more slowly, and still more so of course as it approached more nearly to that of the air, but still in both it was found to correspond. At the end of a hundred and ten minutes the temperature of both rabbits was 84°.

Exp. 68. The foregoing experiment was repeated, with the difference that in both rabbits the lungs were inflated; but we could not perceive that the one rabbit cooled faster than the other.

Exp. 69. Two rabbits, of the same size and temperature, being killed in the usual way, in the one the brain and spinal marrow were wholly removed, the other being left entire. In both the lungs were inflated. We could not perceive that the one cooled faster than the other.

I was particularly careful in repeating these experiments, because they appear at first view to contradict the inferences of Mr. Brodie, to whose labours this part of physiology owes so much. It will appear, however, from what I am about to say, that their result is perfectly consistent with the doctrine maintained by him.

It appears from the foregoing experiments, that after the destruction of the sensorial, the nervous power is still capable of performing all its functions, except that it can no longer give evidence of conveying impressions to the sensorial power, the necessary consequence of the destruction of this power. On comparing these experiments, however, a considerable difficulty presents itself. We have seen it ascertained by those on digestion, that the brain and spinal marrow retain sufficient power after visible death to form secreted fluids. Yet it would appear, from the experiments on temperature, that the influence of the brain and spinal marrow has no effect under the same circumstances in promoting the evolution of caloric, although it is evident that the system still retains the power of evolving it, and from former experiments, that this power depends on the state of the nervous system. The secreted fluids are no longer formed if the influence of the brain be withdrawn, the evolution of caloric takes place in the same way whether the influence of both the brain and spinal marrow be withdrawn or not.

A well-known fact appears to remove the difficulty. Although we have reason to believe, I think, from every observation on the subject, that the brain and spinal marrow are the only sources of nervous influence; yet it is evident that a certain portion of this influence remains in the nerves when separated from these organs, as appears from the contractions excited in the muscles by irritating their nerves under such circumstances. The muscle will thus be made to contract as long as any influence remains in the nerve, but this being once exhausted, the nerve has no means of renewing it. Now the first nervous influence which is employed in the stomach after death is of course that already in its nerves. In proportion as this is exhausted, the brain and spinal marrow are called upon for a further supply. It is evident that they cannot be long so called upon, because there cannot long be any supply of proper fluids. If then, instead of the nerves which belong to the stomach, the whole nerves of the ganglian system terminated in this organ, there is reason to believe, that the supply of fluids, which takes place after death, would never be sufficient to exhaust the nervous influence already in its nerves; and consequently, that in that case it would never receive any part of this influence formed after death; and the same degree of digestion would take place after death whether the influence of the brain remained or not. Now this is precisely what seems to happen with respect to the temperature after death. As long as we can by artificial respiration occasion such a change in the blood as elicits nervous influence, the blood draws it from all the nerves of the ganglian system; and it does not appear, that we can support this change long enough to exhaust the nervous influence already in the nerves, and occasion any farther demand for it. It appears from the above experiments that the greatest evolution of caloric, which can, under ordinary circumstances, be effected after death, is but very inconsiderable. Hence the result is the same whether the brain and spinal marrow exist or not. There is already in the nerves more nervous influence than the blood can use*.

The evolution of caloric occasioned by inflating the lungs after death being so small, may arise from our being able but very imperfectly to imitate natural respiration. It is true that we can imitate it sufficiently to give the arterial colour to the blood, but we have seen that the evolution of caloric does not appear to be connected with the change of colour. It is evidently impossible to proportion the quantity of air thrown in, to the demand for it, which is constantly becoming less, so that we are either supplying too much or too little. In the former case the superfluous quantity can, as far as relates to the temperature, have no other effect but that of reducing it, as happened in the above experiments; but although we could supply air in the due proportion, we should still be very far from being able to imitate natural respiration, from which artificial respiration, among other things, differs in the great pressure to which the lungs are subjected in the latter, in which the ribs and diaphragm are moved by the force of the injected air; whereas in natural respiration the ribs and diaphragm being moved by their muscles, the lungs are subjected to no pressure but that of the atmosphere. The great diminution of nervous influence in artificial respiration constitutes an essential difference between it and natural breathing. It would be worth while, although attended with considerable trouble, accurately to ascertain the effects of passing a stream of galvanism through the lungs while artificial respiration is performed.

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* I have considered the cause of the sensation of heat a substance not a quality, because I regard the former as the more probable opinion. I admit that some difficulties respecting it exist.

A very decisive experiment by Mr. Brodie, related in an addition to the Croonian Lecture above referred to, proves that the change of oxygen gas into carbonic acid gas takes place when the lungs are inflated after decapitation.

It remains for us to inquire, whence it arises that the nervous and muscular powers never long survive the sensorial power.

On the destruction of the sensorial power respiration always ceases. M. le Gallois finds a great difficulty in conceiving why respiration should cease on the removal of the brain.

" Il est donc certain que la vie du tronc n'a
" son principe immédiat ni dans le cerveau,
" ni dans aucun des viscères de la poitrine et
" de l'abdomen ; mais il ne l'est pas moins,
" que tous ces viscères sont indispensables à

“ son entretien. Or, en considérant sous quel rapport ils le sont, les faits énoncés plus haut prouvent évidemment que, quant au cerveau, les phénomènes mécaniques de la réspiration, c'est-à-dire, les mouvemens par lesquels l'animal fait entrer l'air dans ses poumons, dépendent immédiatement de ce viscère. Ainsi, c'est principalement en tant que l'entretien de la vie dépend de la réspiration, qu'il dépend du cerveau; ce qui donne lieu à une grande difficulté. Les nerfs diaphragmatiques, et tous les autres nerfs des muscles qui servent aux phénomènes mécaniques de la réspiration, prennent naissance dans la moëlle épinière, de la même manière que ceux de tous les autres muscles du tronc. Comment se fait-il donc qu'après le décapitation, les seuls mouvemens inspiratoires soient anéantis, et que les autres subsistent? C'est là, a mon sens, un des grands mystères de la puissance nerveuse; mystère qui sera dévoilé tôt ou tard, et dont la découverte jettera la plus vive lumière sur le mécanisme des fonctions de cette merveilleuse puissance.”

This difficulty appears to me to arise from his having regarded respiration as a function wholly depending on a combination of the nervous and muscular powers; whereas it seems evident, I think, that the sensorial power also shares in it. The muscles of respiration are, in the strictest sense of the word, muscles of voluntary motion; we can at pleasure interrupt, renew, accelerate, or retard their action; and, if we cannot wholly prevent it, it is for the same reason that we cannot prevent the action of the muscles of the arm, when fire is applied to the fingers. The pain occasioned by the interruption of a supply of air to the lungs is greater than can be voluntarily borne. Respiration continues in sleep for the same reason that we turn ourselves in sleep when our posture becomes uneasy. It continues in apoplexy for the same reason that the patient generally moves his limbs if they are violently irritated. If respiration continues in apoplexy when no irritation of the limbs, however violent, excites the patient to move them; it arises from the interruption of a supply of air to the lungs producing a greater degree of irritation than any other means we can employ. We know of instances in which the hand has been voluntarily held in the fire, but we know of none where the breathing has been voluntarily discontinued till the lungs were injured. As the insensibility increases in apoplexy, the breathing becomes less frequent; and when the former becomes such that no means can longer excite any degree of feeling, the breathing ceases.

By a certain sensation a wish is excited to expand the chest. This is an act of the sensorium. Till this act take place, the nervous, as well as the muscular, power, by which its expansion is effected, is inert ; it is in vain that these powers remain, if the power which calls them into action be lost. Thus the removal of the brain puts a stop to respiration.

Is it said that the motions of inspiration must be involuntary, because we are in general unconscious of them ? But do we not become more or less so of all habitual acts of volition ? We frequently hear such observations as the following : if I did so, I did it unconsciously. Stop a person who is walking, he cannot tell which leg he last moved ; stop a person who is playing on an instrument, he cannot tell which fingers he last employed ; yet all such acts are strictly acts of volition. If we are reminded of them, we can always interrupt, renew, retard, or accelerate them at pleasure. We have no difficulty in perceiving and changing in any way we please the motions of respiration, when we choose to attend to them ; but as there is no other act of volition so habitual, there is none so apt to escape our attention.

The above explanation of the manner in which the removal of the brain puts a stop to respiration will be readily admitted, I think, when we consider to what part of the brain impressions from the lungs are conveyed. It is evidently to the part where the eighth pair of nerves, which supplies them, joins that part of the brain from which the spinal marrow originates. Now it appears from the experiments in which M. le Gallois removed the brain by slices, that respiration continued till he removed the part of the medulla oblongata in which those nerves originate, and then instantly ceased. In these experiments, however, the power of the muscles of inspiration and the nervous power which excites them still remain, as may be easily ascertained by stimuli properly applied to the spinal marrow. It is the influence of the sensorial power which is lost.

We cannot, perhaps, have a better instance of the distinct operation of the sensorial, nervous, and muscular powers, than in the case before us, although they all here conduce to the same end. We may destroy any one of them and leave the others unimpaired. The destruction of the sensation by which we will to inspire, we have just seen, does not destroy the nervous or muscular power employed in inspiration. By means applied to the muscles of inspiration we may destroy their mechanism without depriving any part of the spinal marrow of its power, or at all impairing the above sensation: and we may destroy the nervous influence which excites these muscles by destroying a certain part of the spinal marrow, while they, as may be ascertained by the application of stimuli, perfectly retain their vigour, and the sensation which excites the wish to inspire, though as in the last case, useless, remains unimpaired; nay, if any two of these powers be destroyed, they leave the remaining power unimpaired. The destruction of the muscles of inspiration, and of the nervous influence which excites them, does not destroy the sensation by which we will to inspire; nor does the destruction of this sensation and the nervous influence at all impair the power of the muscles; and we may destroy the sensation in question, and the power of the muscles, without impairing the nervous influence which excites them. So far from true is the position of M. le Gallois, that the power on which all the motions of inspiration depend, resides in the medulla oblongata.

Much has been written by Whytt and many other physiologists respecting the cause of the first inspiration. I cannot help thinking that the difficulty vanishes, when we regard the muscles of inspiration as merely muscles of voluntary motion. The young animal throws them into action to remove a painful sensation occasioned by the want of that change in the blood, which is produced by the influence of the air in the lungs; a process necessary to the existence of the animal as soon as its connexion with the mother ceases, and which can only be effected by expanding the chest, and thus receiving air into the lungs. It seems to be expanded for the first time precisely for the same reason that the fœtus changes its position for the first time by acting with the muscles of the trunk and limbs. In both cases he endeavours to remove an uneasy sensation, and nature has given him the power to remove it by calling into action certain muscles subjected to the will. The first act of deglutition, if it does not occur in the fœtal state, appears to be an act of precisely the same nature with the first inspiration. In both cases, a certain set of muscles of voluntary motion is thrown into action to satisfy a craving, which had no existence in that state.

It may be objected to this view of the first inspiration, that the animal often breathes before a ligature is thrown round the umbilical chord: but we have no reason to believe, that the secondary change, effected in the blood of the fœtus by the vicinity of the maternal blood of the placenta, although this gives it the florid colour, as may be seen by opening the vessels of the chord, is sufficient for the functions of the perfect animal. One of these functions, which we have reason to believe from many phenomena, as well as from direct experiments, is intimately connected with the change effected on the blood by the air, the evolution of caloric, it is evident, is immediately after birth required to be in a state of much greater activity than in the fœtus, which is surrounded by a medium of its own temperature.

When respiration ceases, most of the pulmonary vessels and left side of the heart are no longer supplied with their proper stimulus; and feel more directly perhaps the debilitating influence of black blood. Their functions, therefore, begin to fail. In proportion as this happens, the blood accumulates in the lungs; and the right side of the heart experiences an increased difficulty in emptying itself. By the operation of these causes, both sides of the heart, in warm-blooded animals, soon lose their power after respiration ceases. The arteries, under such circumstances, it is evident, cannot long supply fluids proper for the purposes of secretion, the nervous and muscular solids, therefore, soon deviate from the state necessary for the functions of life, which at length cease in every part.

The above appears to be the order in which the functions always cease in death, whether it be occasioned by injury of the sanguiferous, or nervous system, or both, with the exception of those cases in which the nervous system is so impressed, as immediately to destroy all the functions. The degree of vital energy required for the sensorial, appears to be greater than that required for the nervous and muscular functions, the sensorial functions always, except in the case just mentioned, in which death is instantaneous throughout the system, ceasing first. Respiration consequently is the first vital function which fails, being the only one to which the sensorial power is necessary.

Bichat has been at great pains to ascertain the effects of black blood on the lungs and other organs. To his experiments on this subject I refer the reader. There are but few parts of the physiological works of Bichat which can be confidently referred to. In general he has allowed his reasonings to go far beyond the evidence afforded by his observations and experiments. I shall take this opportunity of making a few remarks relating to the principal points in which I have differed from him. He was unacquainted with the fact, that the spinal marrow performs its functions independently of the brain*, and therefore did not see the difficulty respecting respiration stated by M. le Gallois, but seems to think that the division of the spinal marrow near the head, occasions death by preventing the nervous influence of the brain from reaching the intercostal muscles and diaphragm. The want of this knowledge leads him into inaccuracies, both in his observations on death, and other passages; which are increased by his not being aware, that the sensorial and nervous powers have no direct dependence on each other*. He is led into more obvious errors, as far as I am capable of judging, in various parts of his works, particularly in those which relate to the passions and the death of the brain, by his not knowing that the heart and blood-vessels may be directly influenced, and even their power directly destroyed, by agents acting either on the brain or spinal marrow; and by his supposing that the ganglions are capable of preparing nervous influence independently of the brain and spinal marrow, a supposition which we have seen contradicted by many experiments, and which Bichat does not attempt to support by any observation or experiment directly bearing on the point.

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* The independence of the spinal marrow on the brain, as far as relates to its power over the muscles of voluntary motion, appears from the experiments of M. le Gallois; and as far as relates to secretion, from experiments laid before the reader in the second section of the seventh chapter of this Inquiry.

These circumstances have even led him into the most striking inconsistencies in his great division of the functions into organic and animal. If the experiments which have been laid before the reader be correct, the sensorial functions constitute the animal, and the nervous and muscular the organic life. To this, it may be objected, that plants and the less perfect animals have no nervous system. Would it not be more correct to say, that the operation of their nervous system is more confined? Wherever secretion is performed, the nervous influence, or a power resembling it, must exist. In order that a being, possessed of the nervous and muscular systems alone, may live in perfect vigour, it is only necessary, that respiration should be performed as circulation is, by powers of involuntary motion. A being so formed, though possessed of all the powers of life, would be wholly unconnected with the external world, except as far as food and the influence of air and light are necessary to its existence; all other intercourse with that world depending on the sensorial power. Such is the life of vegetables, and we have reason to believe, that that of the lowest class of animals differs from it in little else than degree *.

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* It appears, however, from various facts, both relating to the action of the muscles and the formation of secreted fluids, that the functions of neither of these powers are confined to the brain or spinal marrow, both organs partaking of both.

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* I do not mean to say that the change effected on the air by plants is of the same nature with that effected by animals, or that they possess a circulation similar to that of animals; but

An animal of this class approaches as nearly as facts will allow us to suppose, to one possessing merely organic life, according to Bichat's definition of it; yet, in the second section of his sixth article, he maintains, that every thing relative to the passions belongs to the organic life; an inconsistency which alone is sufficient to prove a radical defect in his system. Can the passions belong alone to that life in which they never can be excited, in which they never could operate? Even according to Bichat's definition of organic life, it is common to the animal and vegetable world.

CHAP. XI.

Of the Nature of the Vital Powers.

I HAVE, in the preceding chapter, endeavoured to ascertain the seat and the functions of the muscular, nervous, and sensorial powers, and to shew in what manner they are so connected in the more perfect animals, that none of them can long exist without the others. I

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we know that air is necessary to their existence, that some change in it is effected by them, and that in their vessels or canals there is a continual motion of their fluids.

have called them vital powers, because they are the immediate agents in the production of the phenomena of life.

It was generally supposed, till the time of Haller, that the muscular fibre derives its power from the nervous system. He was the first who maintained that this power resides in the muscular fibre itself. He made many experiments to support his opinion, which have been frequently repeated, and the accuracy of which is now universally admitted. His inferences from these experiments however have, been opposed on this ground, that however carefully we dissect away the nervous filaments supplying a muscle, it must still be admitted that nervous influence may remain in it, either in nerves too small to be removed by the knife, or constituting a necessary part of the muscular fibre itself.

This objection, though frequently urged against the inferences of Haller, he never satisfactorily answered; and it seemed to be countenanced, if not confirmed, by the fact, that when causes of injury affecting the nervous system are of such force as instantly to destroy its power, the power of the muscular fibre is also found to be destroyed. This is well illustrated by many of the experiments of Mr. Hunter, and still more fully by those of M. le Gallois, who, as I have already had occasion to observe, regards it as wholly invalidating the opinion of Haller respecting the power of the musclular fibre.

These objections to the opinion of this great physiologist appear to be removed by the thirty-second Experiment related in this Inquiry, from which it appears that the nervous influence, far from supplying any power to the muscular fibre, acts only as a stimulus to it, and exhausts its power as other stimuli do; and by many experiments related in the first and second chapters of the present Part, which seem to prove that the muscular power is not destroyed, in the cases just alluded to, in consequence of the loss of power in the nervous system, but in consequence of the cause of injury acting on the muscular fibre through that system; for it appears from them, that the nervous power may be as effectually destroyed by other means without at all impairing the power of the muscular fibre. If it be allowed that the experiments here referred to, remove the foregoing objections to the opinion of Haller, this opinion must be allowed to be a legitimate inference from experiment; no other objection to it, I believe, having been regarded as of weight. The power of the muscular fibre, then, is a property depending on the mechanism of that fibre, and in no degree directly depending on the nervous power.

That the nervous is independent of the muscular power has not been questioned; but the sensorial and nervous powers have never been correctly distinguished. As far as I know, M. le Gallois is the only author who has attempted, by experiments, to establish a distinction between them. I have, in the preceding chapter, stated my reasons for not adopting the line of distinction pointed out by that writer, and endeavoured by experiments to ascertain where this line lies. From these experiments, compared with others related in former parts of this inquiry, it appears, that the functions of the nervous influence are those of stimulating the muscles, both of voluntary and involuntary motion, conveying impressions to and from the sensorium, effecting the formation of the secreted fluids, and causing an evolution of caloric from the blood; and that we have reason to believe it capable of all these functions after the sensorial power is withdrawn; that is, after the animal is no longer capable of perception, or any act of volition, and is consequently, in the common sense of the term, dead.

No body has supposed that the sensorial power has any direct dependence on the muscular and nervous powers.

If the foregoing observations be correct, each of these powers has an existence not directly depending on the others. My object, in the remaining part of this chapter, is to inquire how far we are acquainted with the causes which produce their phenomena.

When we say that we are acquainted with the cause of any particular set of phenomena, we only mean that we know them to arise from the same cause which produces other more familiar phenomena. Thus we are acquainted with the cause which retains the planets in their orbits, because Sir Isaac Newton proved it to be the same which produces the other phenomena of gravitation. We are acquainted with the cause of lightning, because Dr. Franklin proved it to be the same which produces the other phenomena of electricity. Can we shew that the phenomena of any, or all, of vital the foregoing ~~living~~ powers depend on the same cause which operates in the production of other phenomena?

With regard to the sensorial power, it seems to require but a moment's reflection to answer this question in the negative. There is no real analogy between the effects of this power and the phenomena observed in any other part of nature. Certain fanciful analogies of this description have, indeed, from time to time been suggested, but while they have pleased in the writings of the poet, by the philosopher they have been justly rejected. To a careful observation and judicious arrangement of the phenomena of the sensorial power, therefore, our study of it must be confined.

With regard to the nervous power the case is very different. The principle which operates in many other instances may be the means of exciting the muscles, of conveying impressions to and from the sensorium, of effecting the formation of the secreted fluids, and of causing an evolution of caloric from the blood. The most subtle of known agents, electricity, naturally suggested itself as the cause of the phenomena of the nervous power; and when voltaic electricity, and its signal influence on the muscular system, were discovered, a material step, it was imagined, had been made towards ascertaining the nature of this power. On more mature reflection, however, it was admitted, that to ascertain that galvanism is capable of exciting the muscular fibre is to go but a very short way towards establishing its identity with the nervous power, a very large proportion of bodies possessing this property; and of late the opinion appears to have been abandoned: nor can it be maintained on any other grounds, than by shewing that this species of electricity is capable of the more characteristic, as well as the more simple, functions of that power.

On comparing the properties of galvanism with the phenomena of the nervous power, the analogy between them seemed to me sufficient to warrant the investigation thus suggested.

We have seen, that by dividing the eighth pair of nerves the power of digestion, and consequently the secretion of gastric juice, is wholly lost, and the secreting power of the lungs also deranged. This appeared to offer an excellent opportunity of ascertaining how far galvanism is capable of effecting the formation of the secreted fluids. It is not difficult, by coating the lower part of the divided nerves with tin-foil, and applying a small plate of metal to the skin over the stomach and lungs, to expose these organs, by means of a galvanic trough, to any degree of galvanic power which we may judge proper. I explained my views to Mr. Hastings, and requested him to make the following experiment *.

Exp. 70. The hair was shaved off the skin over the stomach of a young rabbit, and a shilling bound on it. The eighth pair of nerves were then divided, and about a quarter of an inch of the lower part of each coated with tin foil. The tin foil and shilling were connected with the opposite ends of a galvanic trough, containing fifty-two four-inch plates of zinc and copper, the intervals being filled with muriatic acid and water, in the proportion of one of acid to seven of water. The galvanic influence produced strong contraction of the muscles, particularly of the fore limbs.

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* From my dislike to make experiments on living animals, when I had occasion to make such experiments, I frequently requested my friends to perform the operative part; but I have related none the result of which was not observed by myself.

For five hours the animal continued quite free from the symptoms which follow the division of the eighth pair of nerves in rabbits. It had neither vomited nor been distressed with dyspnœa. It had not eaten any thing after the nerves were divided. At this time the power of the trough became much weaker, so that it produced no visible effect on the muscles. The respiration now began to be disordered. In a quarter of an hour it became so difficult, that the animal appeared to be dying. It was gasping. Acid was put into the trough till the galvanic power became as great as at first. Soon after this the animal ceased to gasp, and breathed with much greater freedom. The galvanic process was several times discontinued and renewed, so that we repeatedly saw the gasping and extreme dyspnœa return on discontinuing, and disappear on renewing it. The animal died in six hours after the division of the nerves.

On opening it we found the œsophagus perfectly natural, and no food in it. The stomach was not larger than usual. The food had undergone a considerable change. The appearance and smell of the parsley were gone. The smell was that of the rabbit's stomach while digestion is going on, which is peculiar. Both Mr. Hastings and myself, who have been much accustomed to examine the stomach of rabbits under various circumstances, thought that digestion was nearly as perfect as it would have been in the same time in a healthy rabbit. This rabbit had not eaten any thing for twelve hours till within three hours of the experiment; it was then very hungry, and was allowed to eat as much parsley as it chose.

The membrane of the trachea was of its natural colour, and there was no fluid in it. The ramifications of the bronchia in the left lung were quite free from frothy mucus. There was some fluid in the right lung, though it did not appear much gorged, there was one dark spot on it. The lungs collapsed imperfectly on opening the chest. I requested Mr. Hastings to make the experiment in the following manner:

Exp. **71.** Two full grown rabbits were kept without food for twelve hours: within half an hour of the experiment, they ate as much parsley as rabbits usually do at one time. After the hair was shaved off the region of the stomach in one of them, and a shilling bound upon it, the eighth pair of nerves were divided about eight o'clock in the morning, and each nerve coated with tin foil, to the extent of half an inch. Difficulty of breathing was evident immediately after the operation. It was nine o'clock before the animal was brought properly under the galvanic influence; till then the respiration was very much oppressed ; it soon improved on keeping up a regular and gentle twitching of the muscles of the chest and fore-legs. It appeared to us, that in the preceding experiment the animal had been exhausted by the galvanic influence. In this instance, we regulated its degree by the effect it produced, using the third, half, or whole of the trough, according to circumstances, regarding a gentle twitching of the fore-legs, as the measure of a due degree of the galvanic power. About ten o'clock the breathing became very laborious, owing to the trough's not acting well ; on increasing the galvanic power the breathing became pretty free, although the animal still made a croaking noise in respiration, which continued more or less till its death. The cause of this I shall presently explain. The breathing always began to get worse when the galvanic power became too weak to produce any twitching in the fore-legs.

At eight o'clock in the evening the animal seemed very composed, and breathed with considerable freedom, the trough having acted regularly for some hours. Notwithstanding it was kept in the lying position on its side, it now ate greedily some bits of parsley offered to it. The same quantity was at the same time given to the other rabbit. There did not appear any increase of the difficulty of breathing, nor any disposition to vomit after taking the food. It was now about twelve hours since the nerves were divided, during which there had not been the least disposition to vomit.

At about a quarter past ten, that is fourteen hours and a quarter after the nerves had been divided, the galvanic influence had become too feeble, and the animal made one attempt to vomit. The power of the trough was increased, and no further attempt to vomit took place. After this, however, the breathing became more laborious, and although it gradually improved very much, it could not be brought to its former state, the animal continuing to gasp slightly. About twelve o'clock it had a severe return of dyspnœa, which from this time to a quarter before one increased rapidly. At one it died, having survived the division of the nerves seventeen hours. The other rabbit, which had been left undisturbed, was killed at the same time.

The stomachs of both were laid open; we found that of the rabbit, in which the nerves had been divided, no more distended than the stomach of the other. The food in it had the appearance which it has in a healthy stomach while digestion is going on. The only differences between the contents of the two stomachs were the following:—The food which the healthy rabbit had taken during the experiment, was found in the cardiac portion of the stomach, and digestion was going on rapidly in it, while that, which the other had taken at the same time, was still in the œsophagus, and consequently unchanged. The position this animal had been in was unfavourable to the food's reaching the stomach. There was about a quarter of an inch of the œsophagus, between the food and the stomach, quite empty. The contents of the middle portion of the two stomachs could not be distinguished from each other, those in the pyloric end only differed in being of a firmer consistence in the healthy rabbit. In both the contents of the pyloric end of the stomach were most digested, and in both the food had equally lost the appearance and smell of parsley, and acquired the smell peculiar to the stomach of the rabbit while digestion is going on. The food in the duodenum was equally digested in both. We have seen, that after the eighth pair of nerves is divided, parsley will remain in the stomach of a rabbit wholly unchanged for six-and-twenty hours. Neither Mr. Hastings nor myself could, from any thing we saw in the stomach of the rabbit which was galvanised in this experiment, have doubted its being the stomach of a healthy rabbit. We here see the influence of the brain removed from a secreting surface, that of a galvanic trough substituted for it, and the result the same as if the influence of the brain had still continued.

If we compare the foregoing experiment with experiment 58, in which only one of the eight pair of nerves was divided, we find the difference of result very striking. In the latter, parsley remained in the stomach unchanged for nearly two days. In the former, although both nerves had been divided, the whole food contained in the stomach, notwithstanding it had lain in it a comparatively short time, was nearly as much changed as in the stomach of a healthy rabbit.

We found the membrane of the trachea of a very deep red colour, but there was not much fluid in it. The lungs did not collapse on opening the thorax, the air cells being full of a frothy and bloody serum. The lungs were externally of an uniform dark red colour. The heart was a little increased in size, and highly vascular. The cause of the peculiar dyspnœa in this experiment, and of the croaking noise, was, that the laryngal nerves were injured by being stretched, in order to divide the eighth pair as near to them as possible. The reader will see from what is said above, that dyspnœa and a croaking noise are the effects of injuring the laryngal nerves. We learn from the state of the lungs after the division of the eighth pair of nerves, why an animal cannot be long preserved by artificial respiration after the brain is removed. M. le Gallois, indeed, observes, that artificial respiration produces the same loaded state of the lungs. Throughout the whole of the body there was a general increase of vascularity. This, as appeared from what was observed in other cases, was the effect of the galvanic influence.

In the foregoing experiments this influence had been directed chiefly to the region of the stomach. In order to see the effect of directing it more particularly to the lungs, I requested Mr. Hastings to perform the experiment in the following manner.

Exp. 72. A full grown rabbit was kept without food for twelve hours. Within two hours of the experiment it was allowed to eat as much parsley as it chose. The hair being then shaved off the skin of the thorax, it was covered with tin foil, but not so low as the pit of the stomach. The eighth pair of nerves were now divided lower down than in the last experiment, and the lower portion of both nerves coated with tin foil for about a quarter of an inch. The laryngal nerves were not disturbed, and no croaking ensued. It was an hour after the operation before the galvanic influence was so applied as to keep up a gentle twitching of the muscles of the chest and fore legs. This effect of the galvanism was kept up uniformly for five hours, during which time there was hardly any dyspnœa.

In about an hour after this, an uniform effect from the trough could not be kept up on account of the tin foil having been torn. In the course of an hour the breathing became much worse, and we could not again get the tin foil generally applied to the chest. It was kept imperfectly applied till the death of the animal, which happened in about two hours and a half after this; that is, about nine hours and a half after the division of the nerves. It seemed to be produced by dyspnœa. The animal had shewn no tendency to vomit.

The lungs collapsed on opening the thorax, though not so perfectly as when they are healthy: they swam in water. The inner membrane of the trachea was redder and more vascular than usual. There was no frothy mucus in it. The bronchial cells, near the great division of the trachea, were full of mucus; but on tracing them further, there was very little, and none at all towards the surface of the lungs, which was of a deeper red than natural, but not shewing patches of red, as where the eighth pair of nerves are divided without the application of galvanism. The heart was much more vascular than natural. The same observation applies to every part of the thorax. The thoracic viscera, in short, were rather in a state of high inflammation, an effect always produced by a considerable galvanic power in the part to which it is directed, of which the animal evidently died, than in that in which they are found after the division of the eighth pair of nerves.

The stomach was larger than natural, and the food but little altered, retaining the colour, smell, and stringiness of the parsley. There was no food in the œsophagus. It is evident that the stomach, in this experiment, was but little exposed to the galvanic influence. It was sufficiently so, however, to prevent the vomiting, and to occasion more change in the food than happens when the nerves are divided without applying galvanism.

The reader may remark, that as the stomach is here found in a state intermediate between that of this organ, when the eighth pair of nerves are divided without the application of galvanism, and when the galvanism is chiefly directed to it; so the state of the lungs in Exp. 70 and 71, is intermediate between what it was in this experiment and what it is when the eighth pair of nerves are divided, without the application of this power.

As the foregoing experiments were made on a granivorous, a carnivorous animal was chosen for the subject of the following experiment, which I requested Mr. Sheppard to perform. I may observe that the three preceding experiments were made in the presence of three, and the following experiment in that of four medical men besides myself.

Exp. 73. Two small dogs, of the same size and age, were kept without food for about thirteen hours; they were then permitted to eat as much lean raw mutton as they chose. In both, the eighth pair of nerves were divided immediately after they had taken the mutton. In one of them the nerves were coated with tin foil, as they had been in the rabbits, a three-shilling piece having been previously bound on the pit of the stomach and lower part of the thorax, after the hair had been shaved off; and galvanism applied as in the foregoing experiments.

We had so arranged the apparatus that the galvanism was applied as soon as the nerves were divided. This is more necessary in the dog than the rabbit, which survives the division of the nerves a longer time, and in which the effects of their division do not so immediately ensue.

The dog which was not galvanised was almost immediately affected with dyspnœa, and within ten minutes with violent and repeated efforts to vomit. The other to which the galvanism was applied of sufficient strength to occasion a very gentle motion in the fore legs, but not any expression of pain, breathed as free as before the division of the nerves, and never made any effort to vomit. The application of the galvanism was twice discontinued for a few seconds, during which the animal breathed very laboriously, but on renewing the galvanism the breathing immediately became free. This dog lived two hours and a quarter. On opening the stomach after death we found the mutton half digested. It had lost its red colour, and was reduced to a soft pulpy substance, in which there was little or no appearance of muscular fibre. That part of the mutton which lay in the pyloric end of the stomach was most digested, which is always found to be the case in the healthy stomach, and affords the best proof that digestion was going on in the usual way. The vessels in some parts of the stomach, and throughout the whole of the small intestines were highly injected, giving those parts a very florid appearance. The lungs were rather redder than natural, but otherwise quite healthy, collapsing perfectly on the thorax being opened.

The other dog was still alive at the end of four hours after the nerves had been divided, but so weak that it could not stand nor move itself from the place where it lay on its side.

It was killed at this time by a blow on the occiput. The mutton, although it had been in its stomach so much longer than in the other dog, was as firm as when it was swallowed, and perfectly retained both its red colour and fibrous appearance, except that on the outside the bits seemed as if they had been dipped in boiling water: immediately below the surface they were quite red. The lungs exhibited the same appearances as those of rabbits under the same circumstances. They were so congested that they collapsed very imperfectly, and their surface was covered with patches of a dark red colour.

There was nothing in the stomach of either dog but the mutton, which was taken at the commencement of the experiment, and no part of it had been thrown into the œsophagus of either. All present at this and the three preceding experiments examined the state of the stomach and lungs, and expressed their entire satisfaction in the results.

It is not to be supposed, that we can, by the artificial application of galvanism, direct it in the proper quantity to the proper parts, in such a way as not to supply more to some, and less to others, than nature supplies. Hence, in the foregoing experiments, inflammatory affections, and irregular action of the muscles, were frequently excited by it. This difficulty, which might easily have been foreseen, and the cause of which is so obvious, cannot weaken the inference from the performance of the more complicated functions of the nervous system by galvanism.

Exp. 74. It was suggested, that, by the power of galvanism, the degree of digestion, which takes place after death, might be increased. But we could not cause the galvanism to produce any appearance of inflammation, either in the stomach or bowels, an effect which the continued action of galvanism always produces in the living animal. The absence of inflammation here seems to arise from the galvanism producing no increased *vis à tergo,* that is, no increased action in the larger arteries. Thus, whatever increase of nervous influence there may be, there can be no increased supply of fluids for it to act upon, without which, it is evident, there can be no increased secretion.

I have no doubt that the slight degree of digestion, after death, which is prevented by dividing the eighth pair of nerves, might, after the division of these nerves, be restored by galvanism. This is an inference from the experiments which have been laid before the reader. To make the experiment would be very laborious, because, for reasons mentioned above, it must be made on a very large scale; and the galvanism continued in each instance as long as the motion of the blood in the capillaries continues after death, probably for three or four hours *.

Exp. 75. Although it is difficult to ascertain whether galvanism influences the state of the stomach after death, the case is very different with respect to the lungs. In them it is easy, from the degree of patching, with precision to ascertain the state of the secretions. The patches are occasioned, we have seen, by dividing the eighth pair of nerves after death. Here, however, there was no occasion for minute observation, for in nine instances, in which a stream of galvanism was sent through the lungs for only about a quarter of an hour after the eighth pair of nerves had been divided immediately after death, all appearance of patching was prevented ; the lungs, after the animal had lain dead about twenty hours, appearing quite sound.

The functions of the nervous influence, we have seen, are to excite the muscles, to convey impressions to and from the sensorium, to effect the formation of the secreted fluids, and to cause an evolution of caloric from the blood. Galvanism has long been admitted to be the best of all artificial stimuli to the muscles, and capable in either direction of passing along the nerves. The foregoing experiments prove it to be capable of effecting the formation of the secreted fluids. It remains to inquire whether it can occasion an evolution of caloric from the blood. To ascertain this point the following experiments were made:

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* Exp. 63.

Exp. 76. A cup was placed in water of the temperature of 98° of Fahrenheit's thermometer, which was ascertained to be the temperature of the rabbit on whose blood the experiment was made, by placing the bulb of the thermometer in the rabbit's mouth, and allowing it to remain there for two minutes. The temperature of all the rabbits used in the following experiments, was ascertained in the same way. Blood was received into the cup from one of the carotid arteries. The bulb of a small thermometer, raised to 98°., and the wires from the different ends of the galvanic trough above-mentioned, the whole trough being charged, were immersed into it. The blood had been in the cup about two minutes before the whole apparatus was arranged. The same quantity of blood taken from the same vessel of another rabbit of the same age and temperature, was received into another cup, placed also in water of the temperature of 98°. So far, however, from there being any evolution of caloric from the effects of the galvanism, the blood in the galvanised cup seemed to cool rather faster than that in the other. The appearance of the blood in the two cups, however, was very different; that in which the wires were immersed assumed a dark venous colour, and most of the coagulum, which had appeared to form more rapidly in this than the other blood, was soon dissolved, the blood again becoming liquid. The blood in the other cup retained the florid colour, and coagulated as usual.

It occurred to me, that the galvanism in this experiment had, perhaps, been applied too late to produce all its effect on the blood. For we must suppose the changes of this fluid to commence as soon as it leaves the vessels; with the assistance of Mr. Hastings, and another gentleman, therefore, I repeated the experiment in the following manner:

Exp. 77. The rabbits were chosen of the same size and temperature, the thermometer in the mouth of each standing at 98°. The cups were disposed as in the last experiment; the water, in which they stood, being at the temperature of 98°. Into the one cup nothing was put but the thermometer; into the other, the thermometer and the two wires from the different ends of the galvanic trough, one on each side of the bulb; the thermometer, raised to 98°, being put into the cups at the moment the blood began to flow. Assistants held the rabbits while Mr. Hastings divided the carotid arteries previously exposed. I observed the thermometer, and a person having a watch marking seconds, noted down the changes of the thermometer, as I mentioned them, and the times at which they took place. The experiment was made first on the blood of the one rabbit, and then on that of the other; but to save repetition, I relate it as if it had been made on both at the same time. The temperature of the mouth is always the temperature of the blood on its first flowing from the vessel. In the cup, where there was only the thermometer, one minute after the blood began to flow into it, the thermometer stood at 97°, in a quarter of a minute more it stood at 96°, and so on gradually falling; for it is to be observed, that although the cups stood in water of 98°, the air in them was more than ten degrees lower.

In the cup, where the wires were, one minute after the blood had begun to flow into it, the thermometer stood at 100°, in half a minute more at 102°, in half a minute more at 100°, in a minute more at 99°, in half a minute more at 98°; that is, in three minutes and a half after the blood had begun to flow into the cup. After this, the thermometer gradually fell.

While the above changes of temperature went on, the blood in the galvanised cup began to assume a dark colour about the positive wire. But it appears from the preceding experiment, that the evolution of caloric was not connected with this change of colour, which took place as quickly where no caloric was disengaged. Besides, the caloric ceased to be evolved soon after the dark colour appeared about this wire, and by keeping up the supply of galvanism, the dark colour continued to extend, till the whole blood in the cup assumed this colour. Air bubbles arose around both wires. Around the negative wire they continued to rise in such quantity as to form a considerable accumulation of froth. All these appearances took place equally, whether the wires were immersed in the blood at the moment it flowed from the vessel, or after the time had elapsed, at which they occasioned an evolution of caloric.

Exp. 78. That I might be assured that we were not deceived in the first experiment, we allowed blood to flow from the carotid artery of a rabbit into a cup placed as above, and after it had remained in the cup only about a minute and a half, during which no change of appearance took place in it, the wires and thermometer were immersed into it. The change of colour, and other phenomena mentioned above, took place exactly as before; but there was no evolution of caloric; the blood continued gradually to cool.

Exp. 79 In a rabbit, whose temperature was only 96°, both carotids were exposed. It was then held over a cup placed in water of the same temperature, and containing the galvanic wires. Both arteries were divided, and the blood allowed to flow into the cup; a thermometer, raised to 96°, being at the same moment placed in the cup between the wires. In a quarter of a minute after the blood began to flow the thermometer rose to 98°, in half a minute afterwards to 99°. In a quarter of a minute more it had fallen to 98°, in a quarter of a minute it was still 98°, in half a minute more 97°, in a quarter of a minute more, that is, two minutes after the blood began to flow, it returned to 96°, after this it continued gradually to fall. The low temperature of this animal, and the evolution of caloric being less than in Experiment 65, probably arose from the same cause.

I wished to ascertain, whether galvanism occasions a similar evolution of caloric from venous blood.

Exp. 80. Blood was taken from the arm of a person, whose temperature, as appeared by putting the bulb of a thermometer into the mouth, was 98°. The blood was received into a cup placed in water of the same temperature, into which were put the wires from the galvanic trough. The thermometer, raised to 98°, was put into the cup as soon as the blood began to flow into it. It continued gradually to sink, at no moment giving the least indication of the evolution of caloric. This experiment was repeated in the same way, and with the same result.

I wished to try the effect of galvanism on blood returning from the viscera.

Exp. 81. For this purpose the vena cava of a young rabbit, whose temperature was rather above 100°, was opened, and the blood from it allowed to flow into a cup, placed in water of rather a higher temperature than 100°; the thermometer raised to the same temperature, and the galvanic wires being placed in the cup, while the blood was flowing. No evolution of caloric whatever took place. As the greater part of the bulb of the thermometer in this experiment remained uncovered, the quantity of blood obtained from this rabbit being small, the experiment was repeated on a cat.

Exp. 82. The temperature of the cat was 97°. This experiment was conducted in exactly the same way as the last, except that the temperature of the water was 97°, and the thermometer, at the time of its introduction into the cup, raised to the same degree. The blood flowed freely into the cup. The thermometer indicated no evolution of caloric. The galvanism produced the same visible effects on the venous as it had done on the arterial blood, except that the colour of the former remained unchanged.

In the seventh volume of the Medico-Chirurgical Transactions, Mr. Henry Earle notices many cases of palsy, in which the temperature of the paralytic limb, although the pulse was good, was lower than that of the rest of the body. In the first case which he mentions, he found that passing the electric fluid through the limb, raised its temperature. I am sorry that I have not had an opportunity of repeating this experiment, either with electricity or galvanism, having not met with any case of palsy, since I read Mr. Earle's paper, in which the temperature was lessened.

Exp. 83. By the foregoing experiments the idea is suggested that some gaseous fluid probably escapes from arterial blood, soon after it leaves the vessel. To ascertain whether this is the case, a glass of such a shape that the smallest globule of air could be seen in it, was filled with and inverted over mercury. A considerable part of the femoral artery of a large rabbit, whose sensibility had been nearly destroyed by opium, was then exposed and divided under the glass. The blood immediately rose into the glass, and was allowed to remain undisturbed for a quarter of an hour, but no gaseous fluid was disengaged from it. In performing this experiment, if great care be not taken, the hair of the animal and hands of the assistants may convey a little air under the glass, by which we were repeatedly foiled in making the experiment. The artery must not be held deep in the mercury, else the weight of the metal by compressing it, will prevent the escape of the blood.

The glass, into which the blood was received, rose only about an inch and a half above the surface of the mercury. Had it risen sufficiently high to take off any considerable part of the pressure of the atmosphere, the experiment, it is evident, would not have been a fair one. What elastic fluids may be disengaged from arterial blood, when that pressure is removed from it, is a different question; it appears from this experiment, that the difference of the effect of galvanism on this blood at the moment it leaves the vessel, and two minutes after it has left it, does not arise from the escape of any gaseous fluid.

Thus we find that galvanism, as it has been found capable of performing the other functions of the nervous influence, is capable also of causing an evolution of caloric from the blood.

Is it possible to explain the results of the foregoing experiments without admitting the identity of the nervous influence and galvanism? We must either admit this conclusion, or maintain that there is another power capable of performing the more complicated and characteristic, as well as the more simple functions of that influence, an inference which is surely at variance with all just rules of reasoning.

When the lungs are deprived of the nervous influence, by which their function is impeded and soon destroyed, when digestion is interrupted by withdrawing this influence from the stomach; can we suppose that any thing will renew the functions of these organs, but restoring to them the influence of which they have been deprived?

Many collateral observations appear to support the inference, which, from the above experiments alone, seems to be unavoidable. The results of experiments, which have been laid before the reader, proving, that the nervous influence bestows no power on the muscular fibre, but acts only as a stimulus to it, and that this influence is capable of acting on the fluids as well as the solids of the animal body; the phenomena of electric animals; the muscles being thrown into contraction by the formation of a circle of the nerves and muscles of different limbs; a pile formed of alternate layers of muscle and brain exhibiting galvanic phenomena, and the vast changes, of a nature similar to those which take place in secretion, which have been effected by galvanism in the hands of able chemists, seem all to countenance the opinion of the identity of this power, and the nervous influence. To which I may add, that Sir Everard Home, in a paper published in the Philosophical Transactions for 1809, relates some experiments, from which it appears, that by the power of galvanism, albumen may be separated from the serum of the blood, both in the solid and liquid form. In the third part of this Inquiry the reader will find many facts, relating to the human body itself, which tend to support the same opinion.

It appears from what has been said of galvanism, that I regard it as wholly distinct from, and having nothing in common with, the vital principle. It is as distinct from this principle as the blood or the muscular fibre is. Like them it is only one of the means subservient to it in the production of the phenomena of life.

We have reason to believe that the first rudiment of life, that is, the first existence of the vital principle, in animals, is in the central parts of the circulation; not, however, in the heart and blood-vessels, but in the blood itself. It is inconsistent with what we observe in the operations of nature, that its first existence should be simultaneous in the muscular fibre, and in the blood. Now we never see the latter formed from muscular fibre, but we constantly see this fibre formed from the blood. Here, as in other cases, it seems to be rather in fluids than solids that the great changes of nature take place.

With regard to the vital principle itself, we may say of it what is said above of the sensorial power. As its effects cannot be classed with the phenomena observed in any other part of nature, we have no reason to believe that we shall ever be able to refer them to any more general principle.

Whether the vital principle be something superadded to bodies, or only a peculiar arrangement of their constituent parts, the fact is, that it bestows on matter certain properties. It is essential that our expressions should convey this fact and no more. The galvanic experiments which have been laid before the reader go far to prove that galvanism has nothing in common with this principle, because these experiments exhibit them, acting parts in the animal economy wholly of a different nature.

I here wish particularly to state, what, although fully expressed in the first edition of this Treatise, has yet been overlooked by some in alluding to my opinions, that the effects observed from galvanism in the above experiments, are its effects on parts endowed with the vital principle, wholly ceasing, and by no means renewable when this principle is extinct. Galvanism seems capable of performing all the functions of the nervous influence in the animal economy, but neither the nervous influence nor galvanism can excite the actions of animal life, except in parts endowed with the vital principle. Parts endowed with this principle collect the nervous influence, and apply it where it is wanted to act on parts also endowed with the same principle; but the nervous influence itself seems to be nothing more than that influence which operates in the production of all galvanic phenomena.

With regard to the muscular power I have had occasion to refer to Mr. Hunter's observations on the analogy which exists between the contraction of the muscular fibre and the coagulation of the blood, and to dwell particularly on one of the most striking of these analogies *.

When the whole of the facts on this subject are duly considered, the conclusion appears to

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* In the effects of galvanism on the blood after it is removed from the body, above described, and its effects on the muscular fibre, we see another striking instance of this analogy.

be unavoidable, that the contraction of the muscular fibre, and the coagulation of the blood depend on the same cause. The close analogy between the coagulation of the blood and that of other coagulable fluids cannot be overlooked; so that it seems a necessary inference that the contraction of the muscular fibre is the effect of the same power by which certain fluids coagulate, and therefore depends on a chemical change. But I would here say of this change, what is said above of the galvanic power, that it is only in bodies possessing the vital principle, that it can produce the phenomena of life. It is only before this principle is extinct, that it can produce contraction of the muscular fibre or spontaneous coagulation of the blood. It would appear from these observations, that the nervous influence or galvanism in exciting the muscular fibre, as in the formation of the secreted fluids, and the evolution of caloric from the blood, operates, by effecting a chemical change. Thus the phenomena of the nervous power seem to be only another field, in which galvanism exhibits those striking chemical powers, which we have seen it display in other instances.

No writer, as far as I know, has attempted to explain the following difficulty respecting the fœtal state. The influence of the brain and spinal marrow we have seen is necessary to the function of secretion, and consequently to the life and growth of the body; but fœtuses have been born alive without either of these organs. To remove this part of the difficulty it has been said, that in such fœtuses the nerves perform the functions of the brain and spinal marrow. This is not only a gratuitous supposition, but opposed by every fact on the subject relating to the perfect animal. If, however, we admit this supposition, it will go but a very short way towards removing the difficulty. How shall we account for the life and growth of the fœtus, when the whole nervous system appears an inorganized substance, and as far as we can see, wholly incapable of its functions; while the sanguiferous system appears to be completely organized, and capable of all its functions? Nay, the heart may be seen performing its functions in the chick *in ovo*, when no vestige of brain or spinal marrow can be traced. What in all these cases supplies the place of nervous influence? What influence co-operates with the sanguiferous system in effecting secretion? Did the evolution of the brain and spinal marrow in the formation of the animal keep pace with that of the sanguiferous system, a difficulty of some weight would still remain. The first rudiment of life must in that case have existed at the same time in the blood and the nervous system.

The experiments which have been laid before the reader do not explain these difficulties; but they suggest an explanation of them, the accuracy of which must be ascertained by farther experiments. If the nervous influence be galvanism, there may be some apparatus in the uterine system for collecting and applying this agent, which is every where diffused, till the brain and spinal marrow can perform their functions, and which may continue to supply their place where they never exist. We have seen that galvanism is capable of performing all the functions of the nervous power, properly so called. In combination with the powers of circulation it can, therefore, perform all the functions essential to the life of the perfect animal except respiration, to which. we have seen, the sensorial power is necessary. It is worthy of remark, that this is the only function, immediately essential to life in the perfect animal, which does not exist in the fœtal state.

We have seen, that in dying the sensorial powers are the first which cease, their continuance seeming to require the most perfect cooperation of the sanguiferous and nervous powers. For the same reason they appear to be the last which are formed.

If the foregoing view of the subject should on investigation be found correct, we must regard the first rudiment of life as confined to the central part of the circulation, from which, by the power of galvanism, collected by some means external to the fœtus, all other parts are gradually evolved, till within the fœtus itself a galvanic apparatus of sufficient power for the performance of the nervous functions is produced; the sensorial functions appearing to be superadded when all the others approach to their perfect state.

A difficulty similar to that we have been considering exists respecting the lowest class of animals in which no nervous system can be discovered, and vegetables. Both of these classes of beings must necessarily possess the secreting power. It is requisite to the formation and growth of their bodies. Mr. Hunter found that the most imperfect animals are capable of occasioning an evolution of caloric.

CHAP. XII.

A Review of the Inferences from the preceding Experiments and Observations.

From the various experiments and observations which have been laid before the reader it appears,

1. That the vessels of circulation possess a power capable of supporting a certain motion of the blood independently of the heart. Exp. 24, 63, 64.

2. That the power both of the heart and vessels of circulation is independent of the brain and spinal marrow. Exp. 1, 2, 3, 4, 5, 6, 7, 8, 9, 10, 11, 12, 13.

3. That the nervous influence is capable of acting as a stimulus both to the heart and vessels of circulation. Exp. 14, 15, 16, 17, 18, 23, 26, 27, 28.

4. That the nervous influence is capable of acting as a sedative both to the heart and vessels of circulation, even to such a degree as to destroy their power. Exp. 16, 17, 18, 19, 20, 21, 22, 26, 27, 28, 29, 30.

5. That the effect of the sedative is not the result of the excess of stimulus, but, like excitement, the direct operation of the agent. See the observations after Exp. 40, and the experiments there referred to *.

6. That the power of the muscles of voluntary motion is independent of the brain and spinal marrow, and that their relation to the nervous

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* A moderate application of every agent appears to act as a stimulus, an excessive application of it, as a sedative. The quantities which act as stimulus and sedative bear no particular proportion to each other, but in different agents exist in every possible proportion.

system is of the same nature with that of the heart and vessels of circulation, the nervous power influencing them in no other way than other stimuli and sedatives do. Exp. 31, 32, and the observations under Exp. 32.

7. That the cause of the muscles of voluntary and involuntary motion appearing at first view essentially to differ in their nature, is their being excited by stimuli essentially different, the former being always excited by the nervous influence, the latter though occasionally excited by this influence in all their usual functions obeying other stimuli. See the observations under Exp. 32.

8. That the brain and spinal marrow act, each of them, directly on the heart as well as on the muscles of voluntary motion. Exp. 18, 19, 23.

9. That the laws which regulate the effects of stimuli applied to the brain and spinal marrow on the heart and muscles of voluntary motion are different. Exp. 33, 34, 35, 36, 37, 38, 39, 40.

10. That mechanical stimuli, applied to the brain and spinal marrow, are better fitted to excite the muscles of voluntary motion, and chemical stimuli the heart. Exp. 33, 34, 35, 36, 37, 38.

11. That neither mechanical nor chemical

stimuli, applied to the brain and spinal marrow, excite the muscles of voluntary motion, unless they are applied near to the origin of their nerves ; and consequently that these muscles are excited by stimuli applied to very minute parts of the above organs. Exp. 33, 35, 37.

12. That both mechanical and chemical stimuli, applied to any considerable part of the brain or spinal marrow, increase the action of the heart, which cannot be increased by any stimulus applied to a minute part of these organs. Exp. 34, 36, 38, 41, 42, 43.

13. That the heart obeys a much less powerful stimulus, applied to the brain and spinal marrow, than the muscles of voluntary motion do. Exp. 35, 36, 37, &c., and observations after Exp. 43.

14. That stimuli, applied to the brain and spinal marrow, excite irregular action in the muscles of voluntary motion. Exp. 33, 35, 37.

15. That no stimulus, applied to the brain or spinal marrow, excites irregular action in the heart or vessels of circulation, nor is their action rendered irregular by sedatives, unless a blow, which crushes a considerable part of the brain or spinal marrow, be regarded as a sedative. Exp. 28, 39, &c.

16. That the excitement of the muscles of voluntary motion takes place chiefly at the mo-

ment at which the stimulus is applied to the brain and spinal marrow, while that of the heart may generally be perceived as long as the stimulus is applied. Exp. 40.

17. That after all stimuli applied to the brain and spinal marrow fail to excite the muscles of voluntary motion, both mechanical and chemical stimuli so applied still excite the heart. Exp. 38.

18. That all the foregoing differences in the effects of stimuli applied to the brain and spinal marrow on the heart and muscles of voluntary motion are referable to the following law: That the heart is excited by all stimuli applied to any considerable part of the brain or spinal marrow, while the muscles of voluntary motion are only excited by intense stimuli applied to certain small parts of these organs. See the observations under Exp. 43.

19. That the function of secretion is destroyed by dividing the nerves of the secreting organs. Exp. 44, 45.

20. That it may be restored after it is thus destroyed by the galvanic influence, Exp. 71, 72, 73, 74.

21. That lessening the extent of the nervous system by destroying the influence of any con-

siderable part either of the brain or spinal marrow, deranges the secreting power. Exp. 54, 55, 56, 57, 58.

22. That dividing the spinal marrow, does not derange the secreting power. Exp. 59.

23. That the vessels of secretion only convey the fluids to be operated upon by the nervous influence. See Chap. 5, and the experiments there referred to.

24. That these vessels, like the vessels of circulation, are independent of, but influenced by, the nervous system. Ib.

25. That the peristaltic motion of the stomach and intestines is independent of the nervous system. Exp. 46, 47.

26. That it is capable of being influenced through it. See the beginning of Chap. 7.

27. That in the stomach of the rabbit, and probably in that of similar animals, the food when received into the stomach remains at rest in the central part of this organ, and unmixed with the food previously taken; and that it is changed in proportion as it approaches the surface of the stomach, in consequence of that, previously there, being moved on towards the pylorus. Exp. 49, 50, 52.

28. That the food is most mixed with the

fluids of the stomach, and the greatest change is effected in it in the cardiac end of the stomach. Exp. 52.

29. That the food is much dryer and of a more uniform consistence, its digestion being further advanced, in the pyloric than in the cardiac end of the stomach. See Exp. 51, and the observations under it.

30. That the efforts to vomit occasioned by the division of the eighth pair of nerves, arise from fresh food coming into contact with the surface of the stomach, no longer covered with its proper fluids. See the observations near the end of the first Section of Chap. 7, and the experiments there referred to.

31. That the muscular power of the stomach remains after the division of the eighth pair of nerves, by which all that part of the food which has undergone the action of the gastric juice is carried into the intestine, undigested food alone remaining in the stomach. Ib.

32. That the secreting power of the stomach is almost as much deranged by destroying a considerable part of the spinal marrow, as by dividing the eighth pair of nerves. Exp. 54, 55, 56, 57, 58.

33. That a similar observation applies to the secreting power of the lungs. Ib.

34. That the stomach and lungs, like the

sanguiferous system, are influenced by every part of the brain and spinal marrow. Exp. 44, 45, 54, 55, 56, 57, 58.

35. That the destruction of any considerable part of the spinal marrow lessens the temperature of the animal. Exp. 54, 55, 56.

36. That the galvanic influence occasions an evolution of caloric from arterial blood, if it be subjected to this influence as soon as it leaves the vessels. Exp. 76, 77, 78, 79.

37. That the galvanic influence occasions no evolution of caloric from venous blood, although subjected to it as soon as the blood leaves the vessels. Exp. 80, 81, 82.

38. That there is no evolution of gaseous fluid from arterial blood on its leaving the vessels. Exp. 83.

39. That, if caloric be admitted to be a substance, its evolution from the blood being effected by the same means by which the secreted fluids are formed, it must be regarded as a secretion. See Chap. 8.

40. That the division of the spinal marrow does not destroy any of the functions of either half of it, (22) the paralysis of the lower part of the body, occasioned by its division, arising from that part having its communication with

the principal source of sensorial power destroyed. See Exp. 59, and the observations on the sensorial power in Chap. 10.

41. That the ganglions are a secondary centre of nervous influence, whose nerves are as extensively distributed as those which proceed from the brain and spinal marrow. See Chap. 9.

42. That the ganglions are the means by which the influence of every part of the brain and spinal marrow is bestowed on the parts, which we have found influenced by every part of these organs. See Chap. 9, and the experiments there referred to.

43. That the influence of every part of the brain and spinal marrow is bestowed on all parts directly or indirectly necessary to the due performance of secretion, this function requiring the influence of every part of these organs (21). Exp. 44, 45, 54, 55, 56, 57, 58.

44. That the position of the ganglions and the distribution of their nerves tend to confirm the view of their use afforded by the above experiments. See Chap. 9.

45. That we have reason to believe, that the great sympathetic nerve arises from the spinal marrow*. Ibid.

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* This position does not rest, as M. le Gallois maintains, on the fact, that crushing any considerable part of the spinal mar-

46. That the proof of the vessels possessing a principle of motion independent of their elasticity, which bears the same relation to the nervous system with the excitability of the heart; not only as far as respects the kind of influence which they derive from that system (43), and the way in which it is supplied to them (42), but also as far as respects the purposes for which it seems to be bestowed on them (43), affords a strong argument for believing that this power is of the same nature with that of the heart. See the Experiments related in the two first Chapters of the present part of this Inquiry, also Exp. 39, 44, 45, 54, 55, 56, 57, 58, and Chap. 5 and 9.

row destroys the power of the heart, while the removal of the brain leaves it unimpaired; because the reader has seen that crushing the brain in like manner destroys the power of the heart, which remains uninfluenced by the removal of the spinal marrow.

It appears to rest on the following circumstances:—the sympathetic nerve is largest about the middle of the spine, becoming less as it ascends and descends. We have found from direct experiment that the thoracic and abdominal viscera are influenced by every part of the spinal marrow, which can only be through the medium of this nerve, the influence of the brain on the other hand, as appears also from direct experiment, being conveyed to these viscera by the eighth pair of nerves. Bichat found that the effects of dividing the eighth pair of nerves in the neck are not increased by dividing in the same place the great sympathetic nerve. See Chap. 9.

47. That the various functions of the animal body may be divided into sensorial, nervous, and muscular. See Chap. 10, and the experiments there referred to.

48. That the sensorial power is not wholly confined to the brain, nor the nervous power to the spinal marrow, both powers in a greater or less degree residing in both organs. See Exp. 60, and the observations which precede and follow it.

49. That the division of the brain into cerebrum and cerebellum seems to relate to the sensorial functions, since the muscles both of voluntary and involuntary motion appear to bear the same relation to both. Exp. 33, 34, 35, 36.

50. That what we call death is the ceasing of the sensorial power alone, the nervous and muscular powers still continuing. Exp. 61, 62, 63, 64, 65, 66, 67, 68, 69, and the observations preceding and following these experiments.

51. That in the function of respiration the sensorial, nervous, and muscular powers are combined. See the observations after Exp. 69, and the experiments there referred to.

52. That it is owing to the ceasing of respiration, that the destruction of the sensorial power is followed by that of the nervous and muscular powers. Ibid.

53. That whatever be the cause of death the functions cease in this order, unless the sensorial or nervous system be so impressed as instantly to destroy all the functions. Ibid.

Such are the immediate inferences from the experiments and observations which have been laid before the reader. By comparing them together we arrive at the following conclusions.

The power of the muscles both of voluntary and involuntary motion is independent of the nervous system, and arises from the mechanism of the muscular fibre itself. Both these sets of muscles are equally capable of being excited by the nervous influence, but while this influence is the sole stimulus to which the muscles of voluntary motion are subjected, it acts only occasionally on the muscles of involuntary motion, which are excited in all their usual actions by stimuli independent of it, and consequently of the will. When the latter muscles are excited by the nervous influence, it is not applied to them in the same way as to the muscles of voluntary motion, to which it is sent directly from the brain and spinal marrow, each muscle receiving its nervous influence from a particular part of these organs; while to the muscles of involuntary motion, it is sent through the great chain of ganglions, each muscle receiving its nervous influence from every part of the brain and spinal marrow.

The excitement of the muscles of involuntary motion in all their usual functions, appears to be rendered independent of the nervous influence, because these functions require a more uniform excitement than could have been derived from this source; and they appear to be subjected to the influence of the whole brain and spinal marrow, because they are directly or indirectly subservient to the function of secretion, which requires for its due performance the influence of every part of these organs; for the nervous influence is not supplied by the brain alone, the spinal marrow supplying a necessary part of it, and that independently of any operation of the brain on this organ.

In the function of secretion, the sanguiferous system appears only to supply the fluids to be operated upon by the nervous influence; and the evolution of caloric, which supports animal temperature, is also effected by the action of this influence on the blood.

We have reason to believe that the nervous influence is the galvanic fluid, collected by the brain and spinal marrow, and sent along the nerves; galvanism being, not only of all artificial means of exciting the muscles, that which seems best adapted to this purpose, but capable of both forming the secreted fluids, and causing an evolution of caloric from the blood, after the nervous influence is withdrawn.

The nervous power is not more distinct from the muscular, than it is from the sensorial power. We find the first capable of its functions after the last is withdrawn.

The only function essential to animal life, in which the sensorial power is concerned, is respiration; and consequently it is by the interruption of this function that the removal of the sensorial power proves fatal, except where the sensorium is so impressed as immediately to destroy all the functions.

The sensorial power appears to be the last which is produced, and the first whose operation ceases.

The foregoing conclusions seem to reconcile all the apparent contradictions stated in the first part of this Inquiry.

The heart continues to act for some time after it is removed from the body, and performs its functions in the fœtal state when neither the brain nor spinal marrow has existed; because it has no direct dependence on the nervous system, and is only influenced by the removal of the brain and spinal marrow in the perfect animal, in consequence of the failure of respiration*.

The heart is supplied with nerves, and subject to the influence of the passions, because, although independent of the nervous system, it is capable of being influenced through it.

Thus, when we remove the brain and spinal marrow, the action of the heart is unimpaired, because it is independent of these organs. When we crush them it is enfeebled or destroyed, because it is influenced through them; and the greater the portion destroyed, and the more sudden its destruction, the greater injury the heart sustains. These facts reconcile the apparent contradictions in the experiments of M. le Gallois.

The heart is independent of the will, because it is exposed to the constantly renewed action of a stimulus, over which the will has no control; and because there is no act of volition which could be performed through the medium of the heart.

The function of the stomach is destroyed by withdrawing the influence of the brain or spinal marrow, while that of the heart is unimpaired; because the function of the heart depends wholly on the muscular power, which we have found in every part of the body independent of the nervous influence; while the function of the stomach chiefly depends on the secreting power, which we have found every where dependent on this influence. As far as the function of the stomach is muscular, it also continues after the nervous influence is withdrawn. The digested part of the food is still carried onwards into the intestine.

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* It is evident from what has been said, that could we perfectly imitate the function of respiration after the removal of the brain and spinal marrow, the heart would soon begin to feel the effects of the general failure of the secreting power; the failure of the secreting power in the lungs, indeed, as I have already had occasion to observe, probably constitutes one of the chief differences between natural and artificial respiration.

The difficulties stated by M. le Gallois respecting the function of respiration, seem to disappear when it is admitted, that, although the muscular and nervous powers, concerned in this function, are, as M. le Gallois states them to be, independent of the brain; the sensorial power is here necessary to call them into action: and that the lungs, being chiefly supplied with nerves from the eighth pair, the sensorial power must, as far as regards them, cease, when that part of the medulla oblongata, from which these nerves originate, and to which all impressions communicated through the spinal marrow must also be sent, is destroyed. The powers of respiration remain after decapitation, but the sensation which excites the animal to call them into action is gone.

PART III.

On the Application of the foregoing Experiments and Observations to explain the Nature and improve the Treatment of Diseases.

A CONSIDERABLE length of time alone can shew how far the principles, which seem to be established by the experiments laid before the reader in the preceding Inquiry, may tend to improve the knowledge and treatment of diseases. It is my intention, in the present Chapter, to point out in what instances they, at first view, appear to promote these ends. I shall begin with the diseases which arise chiefly from a fault in the sanguiferous, afterwards making some observations on those whose cause chiefly exists in the nervous, system. I use the qualifying words of the preceding sentence, because there is hardly any disease of the sanguiferous, whose symptoms do not in some degree depend on the state of the nervous system; and on the other hand, in almost all the diseases of the latter, the sanguiferous system is more or less affected. It is evident, however, from a review of the symptoms of these two sets of diseases, that the nervous more amply partakes of the affections of the sanguiferous system, than the sanguiferous of those of the nervous system; the cause of which is sufficiently evident. We have found that the circulation is immediately necessary to the functions of the brain and spinal marrow; but that those of the heart and blood-vessels may go on for a certain length of time after the former organs have ceased to exist, having only on these organs an indirect dependence through the functions of respiration and secretion *.

Of the diseases of the sanguiferous system there are some in which the force of the circulation is diminished, so that the due supply of blood to the brain fails, producing, according to the degree in which this happens, various symptoms of debility or complete syncope; and others, in which the vessels of this organ are distended with more than their due proportion of blood, either in consequence of the increased action of the heart and large vessels, or of the vessels of the brain being so far weakened that

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* See the experiments related in the first and tenth Chapters of Part II.

their power of resistance is not in due proportion to the usual *vis à tergo*. The two last states produce the same train of symptoms, except that in the former the symptoms of a morbidly increased impetus of the blood throughout the system are more considerable [*]. The species of apoplexy depending on these states of the sanguiferous system is very different from that which I shall soon have occasion to speak of, in which the cause originates in the brain itself. These species, we shall find, are frequently combined; but when apoplexy, from distension of the vessels, exists alone, the brain seems to re-act but little on the sanguiferous system. It is observed above, that I found by experiment, that considerable uniform pressure either of the brain or spinal marrow produces little or no effect on the motions of the heart [†].

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[*] I shall not here enter on the question how far more blood can exist in the encephalon at one time than at another; but only observe that, however incompressible the brain, and immovable the parietes of the head, may be, if the brain is compressed by an increased force of circulation, there must then be more blood, however little, in the encephalon than when the brain is not compressed; and when we consider how many openings there are in the scull, filled only by soft medullary matter, we may easily perceive why there may exist within this cavity very evident accumulations of blood, without corresponding depletion in some other parts of it, the necessity of which certain anatomists have maintained.

[†] Exp. 18.

CHAP. I.

On Sanguineous Apoplexy.

The only change which, in this species of apoplexy, appears to take place in the action of the heart, is, that it becomes slow and oppressed, as if acting against a stronger opposing force, and consequently with greater effort than usual. This is easily accounted for by the circulation through the lungs becoming more difficult, owing to the muscles of respiration being less readily called into action in proportion as the insensibility increases, and the vessels of the pulmonary system being less powerfully stimulated in proportion as the blood less perfectly undergoes the change effected by the air. But this is not the only change which takes place in the lungs in apoplexy. They soon begin to be clogged with phlegm, which, in protracted cases, more than the lessened action of the muscles of inspiration, at length appears to occasion suffocation. This is readily explained by the experiments which have been laid before the reader, from which it appears, that when a considerable portion of the nervous influence is withdrawn from the lungs, the fluids destined to form their secretions, being no longer properly changed, accumulate in them till the air-cells and bronchial tubes are so clogged that their function is at length wholly destroyed*. Now in those cases of apoplexy, in which the brain is so oppressed that the various organs are deprived of a great part of their nervous influence, but not sufficiently oppressed immediately to put a stop to the action of the muscles of respiration, the above change necessarily takes place in the lungs; and as they are of more immediate importance to life than any other organ whose function directly depends on the nervous system, this derangement of the lungs is here the cause of death. We see the patient to the last endeavouring to breathe through the phlegm which clogs them, and at length produces suffocation.

If such be the immediate cause of death in sanguineous apoplexy, we have reason to believe, from experiments† which have been laid before the reader, that by passing a stream of galvanism through the lungs they may be enabled to perform their functions for a longer time than without this aid, and thus the life of the patient for a certain time preserved. There is an evident limit to this effect of galvanism. It is only while the sensibility continues such as to induce the patient to expand his chest to a certain extent and with a certain frequency, that advantage can arise from it. Whatever be the supply of nervous influence in the lungs, if the air is not admitted with sufficient frequency, the proper changes, it is evident, cannot go on. Galvanism, under these circumstances, must fail to relieve the breathing, having no other effect but that of a stimulus in promoting the sensorial functions *.

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* Exper. 44, 45.

† Exper. 70, 71, 72, 73.

On employing galvanism in apoplexy, I had the satisfaction to see the foregoing observations confirmed. After the rattling breathing had come on, and the patient seemed about to be suffocated; he was at least a dozen times made to breathe with ease, the accumulation of phlegm gradually disappearing on the application of galvanism, by which his life was evidently prolonged. The inspirations, about an hour and a half or two hours before death, becoming very imperfect, and less frequent, the galvanism failed to relieve him. The relief obtained, as may be supposed from what has been said, was always of very short duration, the breathing sometimes becoming oppressed as soon as the galvanism was discontinued. I directed it never to be applied for more than ten

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* See the observations on the office of the sensorial power in respiration, in Chap. 10,

minutes at a time, and no greater power to be employed than what I had found a person in health could bear without inconvenience. It appears, from the experiments which have been laid before the reader, that a long continued and powerful application of galvanism excites inflammation. Its proper use in the case before us will appear from what I shall have occasion to say of asthma and suspended animation.

It is evident, from the above observations, that the use of galvanism is not suggested as a means of cure in apoplexy; but it will always, I believe, in the species of this disease which we are considering, prolong the patient's life; and may thus, under certain circumstances, by giving more time for the use of those means which tend to remove the cause of the disease, be indirectly the means of saving it.

Such are the observations which are suggested by the experiments which have been laid before the reader, respecting the action of the heart and the state of the breathing in sanguineous apoplexy. The character of this species of apoplexy seems evidently to arise from the power of the heart being independent of the brain, so that the action of the former seems to be no otherwise affected by the state of the latter, than necessarily arises from impeded respiration[*]. These observations, we shall find, by no means apply to all species of apoplexy.

With regard to the other symptoms of sanguineous apoplexy, we have seen it proved by direct experiment, that the loss of power in the limbs does not here arise from any change having taken place in their muscles, whose power seems equally unimpaired[†] with the muscles of involuntary motion; but from the nervous influence, the only stimulus of the former, being no longer applied to them. The urine and fæces often pass involuntarily; not that any change is produced in the sphincters of the bladder and rectum, but because these being muscles wholly of voluntary motion[‡], although they still retain that degree of contraction which constitutes their state of rest, when the pressure from within increases, as all stimulus supplied to them by the powers of volition is withdrawn, they yield to this pressure.

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[*] See the observations on respiration in Chap. 10.

[†] See the observations after Exper. 32.

[‡] Observations, similar to those which have been made respecting the muscles of inspiration, apply to the sphincters; see Chap. 10. There is no such muscle, as far as I am capable of judging, as what physiologists call a mixed muscle, that is, one partaking of the nature both of the voluntary and involuntary muscles. All the muscles referred to this head are muscles of voluntary motion, but the nature of whose function is such that we are frequently obliged to call them into action.

The species of apoplexy which we have been considering, is the most favourable. By the abstraction of blood the brain is relieved from pressure, and its functions are restored, and continue, unless, as frequently happens, especially where the patient has suffered from previous attacks of the disease, the vessels again yield to the *vis à tergo.* We thus often see the symptoms relieved by blood-letting, but soon recur, and continue to do so till the powers of the constitution are exhausted.

There are two species of apoplexy, proceeding from the same cause with the disease we are considering, but very different both in their nature and prognosis; I mean those in which the distension of the vessels has occasioned rupture, or an effusion of serum. These cases, of a much more fatal tendency, are distinguished from those of mere distension, with great difficulty. The only diagnostics on which it has appeared to me we can rely are, that in the two first cases, particularly in the first, the symptoms generally increase more rapidly than in apoplexy from mere distension, and are little, if at all, relieved by blood-letting. That form of serous apoplexy, which is the effect of general debility, and takes place with little or no congestion of the vessels of the brain, may, for the most part, be distinguished by the habit of the patient, and by the tendency to effusion in other parts. To enter further on these cases, and the diagnosis between them and the different forms of hydrencephalus, would be foreign to my present purpose. I have enumerated them that the reader may the more clearly understand what I mean by nervous apoplexy, on which I shall soon have occasion to make some observations.

It will be necessary here to say more of another affection of the head which insensibly runs into apoplexy from distension; I mean phrenitis. In both we find the vessels of the brain preternaturally distended, and in many cases can detect no other morbid appearance; yet their nature must essentially differ. In the one, the patient often resembles a furious maniac, while insensibility is always the characteristic of the other. This subject leads me to consider the most important of all the diseases of the sanguiferous system, perhaps I may say, of all the diseases to which we are liable.

CHAP. II.

On Inflammation.

I ENDEAVOURED many years ago to ascertain the state of the vessels in the various stages of inflammation, both in the warm and cold-blooded animal, by observing them with the assistance of the microscope. After stating briefly the points which appear to be ascertained by these experiments, a detailed account of which, I have already had occasion to observe, is given in the Introduction to the second part of my treatise on Febrile Diseases, I shall endeavour to ascertain from the result of experiments which have been laid before the reader in the preceding Inquiry, what share the nervous system has in producing the symptoms of this disease.

In the first experiment on the vessels, the inflamed web of the foot of a frog was brought before a microscope; and it was observed, that where the inflammation was greatest, the vessels were most distended, and the motion of the blood was slowest. The distension of the vessels, which in the healthy state admit only the colourless part of the blood, was apparent; for a much greater number of vessels admitted the red particles in the inflamed than in the sound part, and the interstices of the inflamed vessels appeared more opaque, either from the enlargement of innumerable small vessels, still too small to admit the grosser parts of the blood, or from an effusion of its more colourless parts *. With a view to excite the vessels of the inflamed part, I wetted it with spirits, and directed on it the concentrated rays of the sun from the concave reflector of the microscope. In proportion as I succeeded by these means in increasing the velocity of the blood, the diameters of the vessels were diminished, their interstices became more transparent, and the appearance of inflammation was in the same proportion lessened.

In the second experiment I observed the inflammation from its commencement. The fins and tail of the lampern became inflamed by exposure to air. By bringing the former before the microscope, I observed the circulation

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* Dr. Lubbock and Mr. Allen, without having made any experiments on the subject, and guided merely by the phenomena of the disease, maintained, about the year 1790, that inflammation arises from a debility of the vessels of the part. Some hints of this opinion are to be found in writings of an earlier date, but the above gentlemen, in the discussions of the Medical Society of Edinburgh, were the first who brought it forward in a connected form. Neither, as far as I know, has published any thing on the subject; I cannot, therefore, say how far their sentiments, in the detail of the opinion, correspond with mine.

became more languid and the vessels enlarge as the inflammation came on. The motion of the blood in the most inflamed parts at length ceased altogether. By gentle friction and the application of distilled spirits, I repeatedly succeeded in accelerating the motion of the blood in the inflamed parts. In proportion as this happened, the vessels became paler, and the inflammation was evidently diminished.

These experiments having been made on the cold-blooded animal, the mesentery of the rabbit was chosen for the subject of the next experiment. The result was still the same. As soon as the inflammation began, the vessels began to enlarge, and the motion of the blood became languid ; these changes going on till in the most inflamed parts the vessels were enlarged to several times their original diameter, and the motion of the blood ceased altogether. I repeatedly occasioned debility of the capillaries of particular parts of the mesentery by irritating them, and thus saw inflammation rapidly excited by the *vis à tergo* distending the debilitated vessels.

It appears, from these experiments, that the state of the smaller vessels in an inflamed part is that of preternatural distension and debility. That of the larger vessels may be ascertained without the aid of the microscope. We readily perceive, on viewing an inflamed membrane, that they do not suffer a similar distension, and the increased pulsation of the arteries sufficiently evinces their increased action. In inflammatory affections of the jaw and the head, for example, a greatly increased action of the maxillary and temporal arteries is readily perceived by the finger. It is to be observed, however, that, although inflammation, as was evident from the foregoing experiments, begins in the capillaries, if it continues, the circulation in the smallest vessels becoming very languid, those immediately preceding them in the course of circulation begin to be distended, and consequently debilitated. Thus when the lampern was first exposed to the air, the inflammation in the fins and tail assumed the appearance of a slight blush, in which it was difficult with the naked eye to discover any vessels; but after some time had elapsed, vessels of a considerable size were seen passing through the inflamed parts. It is evident that this cannot go very far, because when the arteries preceding the capillaries have lost their power, the circulation is no longer in any degree supported in the latter, and gangrene soon ensues.

The difference between what is called active and passive inflammation seems to depend on the degree in which the arteries supplying the *vis à tergo* to the debilitated vessels are excited.

In short, inflammation seems to consist in the debility of the capillaries, followed by an increased action of the larger arteries; and is terminated by resolution, when the capillaries are so far excited, and the larger arteries so far weakened, by the preternatural action of the latter, that the power of the capillaries is again in due proportion to the *vis à tergo*.

Thus far, I cannot help thinking, the nature of inflammation appears sufficiently evident. The motion of the blood is retarded in the capillaries in consequence of the debility induced on them; an unusual obstacle is thus opposed to its motion in the arteries preceding them in the course of circulation, which are thus excited to increased action. Several difficulties, however, remain, on which the experiments just related throw no light. Why does a failure of power, of small extent in the capillaries of a vital part, strongly excite not only the larger arteries of the part affected, but those of the whole system; while a more extensive debility of the capillaries of an external part excites less increased action in the larger arteries of that part, and often none at all in those of the system in general? Why does inflammation often move suddenly from one part to another, when we see no cause either increasing the action of the capillaries of the inflamed part or weakening those of the part now affected? Why does inflammation often arise in parts only sympathetically affected, and consequently far removed from the offending cause? Why is inflammation often as apt to spread to neighbouring parts, between which and the part first affected there is no direct communication of vessels, as to parts in continuation with that part?

These phenomena, it is evident, are referable to the agency of the nervous system, and seem readily explained by the experiments which prove that the effects of both stimuli and sedatives acting through this system are felt by the vessels, and that, independently of the intervention of any effect produced on the heart[*]. Thus the irritation of the nerves of the inflamed part may excite the larger arteries of this part, or of distant parts, or of the whole sanguiferous system. It will, of course, be most apt to do so where the irritation excited by the inflammation is greatest, and consequently in the more important vital parts. It cannot appear surprising that inflammation should suddenly cease in one part and attack another, when we know that the nerves are capable of exciting to due action the capillaries of the one part, and in the other of impairing the vigour of those which have not yet suffered. In the same way we account for parts only sympathetically affected becoming inflamed, and for inflammation readily spreading to neighbouring parts, which always sympathize, although there is no direct communication between them, either of vessels or nerves.

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[*] Exp. 27, 28.

From the foregoing view of inflammation the principles on which its treatment is founded are obvious. All the local means are calculated either to lessen the contents of the morbidly distended vessels, or to excite these vessels to expel them. The general means are regulated by the effects produced by the disease on the more distant vessels, through the medium of the nervous system; the objects of this part of the treatment being neither to allow the action of these vessels to fall so low that it is incapable of supporting any degree of circulation in the debilitated vessels, nor to become so powerful as farther to distend by gorging them with blood. Thus, when the symptoms of active inflammation run high, we lessen the *vis à tergo;* when gangrene is threatened, we increase it.

When the inflammation is of great extent, or in a part of great importance, the whole sanguiferous system appears, in consequence of the impression made on it through the nervous system, to embrace its contents with greater force than usual, apparently for the purpose of supporting the circulation in the debilitated part. Hence appears to arise the hard pulse, from the degree of which we may generally judge of the degree of the inflammation.

Mr. Hunter's observations, in his work on inflammation, as nearly correspond with the foregoing view of this disease, as the unassisted eye and the pre-conceived opinions, which he had, in common with the rest of the medical world, adopted, admit of. "The vessels," he observes, "both arteries and veins, in the inflamed part, "are enlarged, and the part becomes visibly "more vascular, from which we should sus"pect that instead of an increased contraction, "there was rather what would appear an in"creased relaxation of their muscular powers, "being, as we might suppose, left to the elas"ticity entirely. This would be reducing them "to a state of paralysis simply, but the power "of muscular contraction would seem to give "way in inflammation, for they certainly dilate "more in inflammation than the extent of the "elastic power would allow; and it must also "be supposed that the elastic power of the ar"tery must be dilated in the same proportion." Thus far the reader would suppose that Mr. Hunter was detailing his observations for the purpose of supporting the doctrine which I have endeavoured to establish. He proceeds, however, with the ingenuity which characterizes all his opinions, to an attempt to reconcile the foregoing appearances with the common doctrine of inflammation. " The contents of the " circulation being thrown out upon such oc- " casions, would, from considering it in those " lights, rather confirm us in that opinion; and " when we consider the whole of this as a ne- " cessary operation of nature, we must suppose " it something more than simply a common re- " laxation; we must suppose it an action in " the parts to produce an increase of size to " answer particular purposes, and this I should " call the action of dilatation, as we see the " uterus increase in size in the time of uterine " gestation, as well as the os tincæ in the time " of labour, the consequence of the preceding " actions, and necessary for the completion of " those which are to follow*." The reader will perceive that all the facts recorded in the preceding quotations are in favour of the opinion I have defended, the explanation alone being in opposition to it.

A little before Mr. Hunter observes, " as the " vessels become larger and the part becomes " more of the colour of the blood, it is to be

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* Mr. Hunter's Treatise on the Blood, Inflammation, and Gunshot Wounds, p, 282.

" supposed there is more blood in the part; and
" as the true inflammatory colour is scarlet, or
" that colour which the blood has when in the
" arteries, one would from hence conclude,
" either that the arteries were principally
" dilated, or at least, if the veins are equally
" distended, that the blood undergoes no
" change in such inflammation in its passage
" from the arteries into the veins, which I think
" most probably the case; and this may arise
" from the quickness of its passage through
" those vessels." How different would have been Mr. Hunter's inferences, if instead of trusting to the unassisted eye, he had viewed the inflamed vessels through the microscope. He could then have seen the blood moving, and would have found, that instead of its passage being quickened in the inflamed vessels, it is uniformly rendered slower in proportion to the degree of the inflammation, and in the most inflamed parts, stands still altogether. I have, in the part of my Treatise on Fevers above referred to, shewn from several facts ascertained respecting the colour of the blood, that, within certain limits, the accumulation of this fluid in the debilitated vessels of the inflamed part necessarily causes the blood to retain the florid colour.

It is worth while to observe the difficulty which Mr. Hunter experiences in attempting to explain, on his view of inflammation, the cause of the throbbing pain of an inflamed part. which is evidently a necessary consequence of a debilitated and distended state of the smaller, and increased action of the larger vessels.— " This pain increases," he observes, " every " time the arteries are dilated, whence it would " appear that the arteries do not contract by " their muscular power in their systole, for if " they did we might expect a considerable " pain in that action, which would be at the " full of the pulse. Whether this pain arises " from the distension of the artery by the force " of the heart, or whether it arises from the " action of distension from the force of the artery itself, is not easily determined; we know " that diseased muscles give much pain in their " contraction, perhaps more than they do when " stretched *."

Dr. Parry, in his Elements of Pathology and Therapeutics, takes a view of the nature of inflammation, as far as I know, peculiar to himself. He makes the following observations on the opinion of inflammation, which I had endeavoured to support in the Treatise just referred to. " Neither will this conclusion be " invalidated were it even proved, according " to the opinion of Dr. Wilson, that the velo-

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* Page 287.

"city of the blood in the vessels of an inflamed part is diminished, unless it be also "proved that the velocity is diminished in a "greater proportion than the quantity is increased*." According to Dr. Parry's view of the nature of inflammation, it consists in an increased momentum of the blood in an inflamed part. I should be happy to consider particularly every step of the ingenious, though, as it appears to me, fallacious, train of reasoning, by which he arrives at that conclusion. But as this would lead into a discussion of considerable length, I shall confine myself to the statement of such facts as appear to be incompatible with it†.

As Dr. Parry admits that there is a greater quantity of blood in the vessels of an inflamed part than in the same part when sound, he admits that the vessels in inflammation are morbidly distended, the necessary inference from which is that their power is lessened. This inference did not escape Dr. Parry, but he maintains, in the 198th paragraph, that the blood is moved in the capillary vessels not by the power of these vessels, but by the impulse it receives from the heart.

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* Vol. 1, p. 84.

† Some of the observations which I am about to make were published in the sixth volume of the Medical Repository.

This opinion it is not difficult to submit to the test of direct experiment. We have seen it ascertained by the assistance of the microscope, both in warm and cold-blooded animals, that the motion of the blood in the smaller vessels continues for a long time after what we call death, although immediately after it, a ligature be thrown round all the vessels attached to the heart*. Dr. Parry ascribes the continuance of the motion of the blood in the capillaries, in certain experiments of Haller, after the aorta had been secured by ligature and removed from the heart, to the contractile power of the larger arteries; and he has, in a work published since that above referred to, entitled "*An Experimental Inquiry into the Nature, Cause, and Varieties of the Arterial Pulse, &c.*" made many interesting experiments for the purpose of ascertaining the degree of this power. That something must here be ascribed to it, cannot, I think, be denied; but that the motion of the blood in the capillaries chiefly depends on their own powers, appears from the following facts, which I have ascertained by repeated experiments.

When the power of the capillaries is destroyed, the *vis à tergo*, even in the living animal, as the reader has seen in the experiments on the state of inflamed vessels, and in those in which solutions of opium and tobacco were injected under the skin*, is not capable of propelling the blood through them. He has also seen, that stimulating the debilitated vessels increases the velocity of the blood in them †. As the motion of the blood in the smaller vessels begins to fail, where there is no inflammation, either in the living or in the dead animal, it is observed to stop and go on, to move backwards and forwards in the same vessel, and to stop in some vessels of the same part sooner than in others; phenomena which, it is evident, could not arise from the contractile power of the larger arteries. Besides, it appears from Exp. 62 and 63, that after the power both of the heart and large vessels is lost, the motion of the blood in the capillaries still continues. I need not say how inconsistent this result is with the above opinion of Dr. Parry and other physiologists, that the circulation is supported by the power of the heart alone. Is not the nature of the circulation in the liver sufficient to refute this opinion?

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* Exp. 24, 25, 75, 76.

I have laid before the reader many experiments of a different nature, the results of which appear to be wholly incompatible with the opinions of this author; and which, had there been no other experiments on the subject, would, as far as I am capable of judging, prove that the motion of the blood in the capillaries neither depends on the impetus given to it by the heart, nor on the contractility of the larger vessels I wish the more to insist on this subject, because the authority of Dr. Parry has, with many, given a currency to his opinions. Can all contractility of the larger arteries, the greater part of which, according to Dr. Parry's experiments, consists in the mere property of elasticity, be destroyed by crushing the brain or spinal marrow? We find, from experiments 29 and 30, that the immediate effects of crushing either, is that of instantly destroying the circulation in the capillaries. In these experiments, it is true, no ligature was thrown round the vessels attached to the heart, but the result could not arise from loss of power in that organ, because the total removal of it, either in the warm or cold-blooded animal, produces no such effect. (Exp. 24, 62, 63.) It is an effect altogether analogous to what takes place in the heart itself, from the same cause. (Exp, 20, 21.) When tobacco was applied to the brain the motion of the blood in the capillaries was lessened and soon ceased. (Exp. 26, 27.) Can we ascribe this to the diminished action of the heart? Its total removal, we have just seen, produces no such effect. Nobody will maintain that it is to be ascribed to the tobacco, applied to the brain, destroying all contractility in the larger arteries. Is it possible, from these experiments, to make any other inference than that the capillaries possess a power similar to that of the heart, which is influenced by affections of the nervous system in the same way *.

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* See page 133.

† See the above experiments on inflamed vessels.

The motion of the blood in the capillaries, after visible death, seems so far from depending on the elasticity of the larger arteries, that, as far as I am capable of judging, the emptiness of these arteries after death, may be shewn to arise from the continued action of the former set of vessels.

Dr. Parry found that the larger arteries have their diameter lessened after death, but that it again enlarges, though not to the same extent. I should be inclined to explain these phenomena, if, indeed, they at all obtain in a sensible degree, except when the artery is exposed to the influence of the air, in a way different from that proposed by Dr. Parry. We must suppose, I think, from the facts just mentioned, that the action of the capillaries combines with the contractile power of the larger arteries, in lessening the contents of the latter. As long

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* See Part II. Chap. XII. paragraph 46.

as these contents are of sufficient bulk to stimulate the vessel it will closely embrace them, and thus as its contents are lessened, contract beyond the effect of its elasticity; but by the continued action of the capillaries, the bulk of these contents at length becoming too small to stimulate the vessel, it will be relaxed, and thus, by its elastic power, regain a larger diameter. I have already had occasion to make some observations on this subject*, and to remark how readily the action of the capillaries, after that of the heart has ceased, may lessen the contents of the larger vessels, the sum of the areas of the branches of vessels always exceeding the area of the trunk. That the emptiness of the arteries after death, for they are sometimes found quite empty, does not arise from their contractile power, as Dr. Parry supposes, appears even from his own very accurate experiments. They do not teach us that the contraction of the arteries after death is sufficient to obliterate their cavities, and no less degree of it, it is evident, can wholly expel their contents.

With respect to some other inferences which Dr. Parry makes from his experiments, I think he would admit, that we may be deceived respecting the usual action of the arteries, by dividing and making other experiments on them, while exposed to the air, which applies a peculiarly strong stimulus to parts not usually subjected to its action. If the abdomen of a warm-blooded animal be opened soon after death, although the usual effect is an increase of the peristaltic motion in those parts of the intestines which are exposed to the air, sometimes they fall into a state of permanent contraction, and in this state remain motionless. May not such a permanent contraction, existing in so small a degree as to escape observation, be the cause of Dr. Parry's not having observed any alternate contraction and dilatation in the exposed arteries? Arteries are often very evidently lessened in diameter, and sometimes thrown into strong partial contractions by exposure to the air*. It must, however, be admitted, I think, that the pulse is chiefly† caused in the way Dr. Parry has so well explained. The beatings of the arteries of rabbits, observed in my experiments, were probably of the same nature with the motion of the arteries corresponding to the contractions of the ventricle observed by Dr. Parry. This motion of arteries may often be seen in the human body, in the wrists, the temples, and the neck, while the skin is entire.

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* Part II. Chap. X.

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* See Dr. Parry's 13th, 24th, and 26th Experiments, and the account of an experiment of Vershuir in Dr. Fowler's Thesis on Inflammation. I have frequently observed a general lessening of an artery on its exposure to the air.

† The word chiefly, as here used, seems to require some explanation. All admit that the arteries are elastic tubes, and it will not be denied that the larger arteries are exposed to a more powerful distending force during the systole than during the diastole of the heart. It necessarily follows, therefore, that these arteries are more distended during the former than during the latter, even on the supposition that the increased stimulus of distension excites no vital action in them. Whether the increased distension is in such a degree as to be sensible is another

When we consider attentively the results of the various experiments to which I have referred, does it not seem a necessary inference that the blood is moved in the capillaries by the power of these vessels themselves; and, consequently, that if they are debilitated, the momentum of the whole blood in the part, as well as its velocity must be less than in health? The truth of this inference appears, indeed, from direct experiments; for from those made with a view to ascertain the state of the vessels

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question. The experiments of Dr. Parry prove that it is not sensible to the eye when the artery is exposed. That it is sensible to the touch appears, I think, from the circumstance, that when the circulation is vigorous the pulse is sensible to pressure too slight to influence the caliber of the artery. It is sensible to the slightest touch. But that the sensation produced in the finger in feeling the pulse, is excited chiefly in the way Dr. Parry has explained, appears from its increasing with the pressure until the latter becomes such, as nearly to obliterate the cavity of the artery.

in an inflamed part, of which an account has been given, it clearly appears not only that the velocity of every part of the blood was lessened, (which Dr. Parry admits may be the case, supposing the lessened momentum, arising from this cause, more than compensated by the increased quantity of blood,) but that the general momentum of the blood also in the inflamed part was lessened; because the blood was observed to move more and more slowly, till in the most inflamed parts it ceased to move altogether. Now before the momentum of the blood in those parts was wholly lost, it must have passed through all the degrees between the healthy momentum and none; during which the part exhibited the phenomena of inflammation.

In the introduction, above referred to, I have pointed out in detail, that the view of inflammation there taken is supported by the various phenomena of that disease.

We may easily, I think, from what has been said, perceive the steps by which inflammation terminates in resolution and in gangrene. In the one case, the debilitated capillaries are excited to due action by the increased action of the larger arteries; in the other, the increased stimulus failing to produce this effect, the capillaries wholly lose their power, and the part becomes subject to the laws of dead mat er. The process of suppuration is more complicated, and between the inferences from the experiments which have been related respecting the state of the vessels in an inflamed part, and those afforded by the experiments of Sir Everard Home* on the formation of pus, related in his valuable Treatise on this fluid, there is a chasm which must be filled up by future observation. It appears from what has been said, compared with the experiments of Sir Everard Home, that when the capillary vessels of a part remain for a certain length of time in a state of debility and distension, it often begins to secrete a fluid which becomes pus; for Sir Everard has shewn that this fluid has not the purulent appearance when first secreted, but acquires it while it remains on the inflamed surface, and does not acquire it the less readily when removed from that surface in a colourless state, provided its proper temperature be preserved, and it is equally exposed to the influence of the air, which promotes the change. He has, in the above publication, thrown great light on the Pathology of some of the most important internal diseases, by showing how readily pus is formed by secreting surfaces independently of any breach of substance. He found it completely formed by causes of irritation applied to such surfaces in the short space of five hours. Whether this fluid is secreted from the contents of the original vessels, or, as Mr. Hunter supposes, of a new set of vessels formed in the diseased part, we cannot tell. We are also unacquainted with the nature of the process by which the diseased parts are removed in the formation of abscess. We know not whether it be by an increased action of the absorbents of the part, or by the action of vessels formed for the purpose. We cannot suppose that the diseased parts are melted down and assimilated into its own nature by the action of pus, an opinion at one time prevalent, since we find that this fluid with all its properties may be formed by inflamed surfaces, without any loss of substance taking place; and it more directly appears, from the experiments of Sir Everard Home, that pus does not possess the property of eroding the solids. These topics open a fruitful and interesting field of inquiry. By patient observation, and the aid of glasses, it is not improbable, in the present improved state of chemistry, that the whole process of suppuration might be unfolded.

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* A Treatise on the properties of Pus, by Everard Home, Esq. 4to. London, 1788. This Treatise was re-published in 1797, in his work on Ulcers.

When the larger vessels of a part are debilitated and consequently distended without previous distension of the capillaries; the disease, which may be termed congestion or partial plethora, is of a nature very different from inflammation. In this case there is little or no distension of the capillaries, as appears from their being pale or only slightly turgid with red blood. The *vis à tergo*, from the debilitated state of the larger vessels, being too weak greatly to distend them, they more or less perfectly retain their power, and as long as the larger vessels can afford any supply of blood, preserve the circulation. We have just seen that they can support the motion of the blood, both in the warm and cold-blooded animal, long after the effect of the powers of the larger vessels has ceased. Such appears to be the state of the vessels of the brain in sanguineous apoplexy, while in phrenitis the larger vessels are comparatively little distended, the distension being chiefly in the capillaries. This difference is evident on dissection. After the latter disease, when it has been distinctly formed, a general blush is observed in the parts of the brain affected; while, after the former, a preternatural distension of the larger vessels is conspicuous, while the brain itself is often nearly or altogether of the natural colour. It is an observation of writers on phrenitis, that if coma supervene on delirium in this disease, it is almost always fatal. The cause of which is evident from what has been said. If, while the capillaries are debilitated, the larger vessels to a considerable degree also lose their power, the circulation in the former must wholly fail.

In other parts, as well as in the brain, we constantly observe, that the distension of the capillaries is attended with acute symptoms, great pain and fever, while that of the larger vessels is generally attended with little of either, being chiefly denoted by a failure in the function of the part affected. The cause of this difference appears from those experiments which prove that the sanguiferous and nervous systems sympathize in their extreme parts in a way they are not found to do in any other [*]; which we have reason to believe arises from the capillaries supplying to the nervous influence the fluids on which it operates in the function of secretion [†], the failure of which must necessarily occasion a degree of derangement in the nervous system, which cannot arise to the same degree from causes chiefly affecting the larger vessels; for however debilitated these vessels may be, unless the circulation in them fail altogether, in which case the death of the part soon ensues, the capillaries, as appears from what has just been said, are still capable of affording a certain supply of fluids to the secreting power.

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[*] Exp. 44, 45.

[†] Chap. 5.

It has long been observed by physicians, that the inflammation of the same organ sometimes excites acute pain and a great degree of fever, and in other cases comparatively little of these symptoms, being chiefly remarkable by the lesion of function it occasions. Thus inflammation of the brain has been divided into two species—phrenitis and phrenismus; the latter differing in no essential respect from sanguineous apoplexy—that of the lungs into pleurisy and peripneumony, &c. The difference of the symptoms in such cases has been explained by the supposition that, in the acute cases, the membrane is affected, and in those less acute the paranchyma. Numerous dissections have now proved the fallacy of this explanation. The paranchyma alone having often been found affected in the most acute, and the membranes alone in the least acute cases*. I believe it will often appear that in the former the capillaries, in the latter the larger vessels, are the chief seat of the disease. I am aware that this

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* If the reader will consult the 20th Epistle of Morgagni *De Sedibus et Causis Morborum,* particularly the 9th, 33d, 35th, 39th, 41st, 43d, 47th, 49th and 62d sections of it, and some parts of his 21st Epistle, he will find that the symptoms regarded as peculiar to pleurisy have frequently attended the paranchymatous inflammation of the lungs, and that when the pleura was not at all affected. When we inspect the bodies of

those who die of inflammation of the lungs (says Schroeder Opusc. Med.) they alone are sometimes found inflamed, although the symptoms of pleurisy had been well marked. Petrus Servius opened three hundred people at Rome, who died with the symptoms of pleurisy, in which the lungs were greatly inflamed, the pleura little or not at all. Tissot met with similar cases; and Diemerbroech says, that in two or three cases, in which there had been no acute pain, and where consequently, according to the common opinion, the paranchyma of the lungs alone should have been found affected, the pleura equally partook of the disease. Burserius, observes, that dissections are not wanting to prove that inflammation of the pleura has been present without any pain. Sydenham seems to go so far as to believe the paranchyma of the lungs to be very frequently the seat of pleurisy. And Juncker, in his Conspectus Pathologiæ, observes that pleurisy often passes into peripneumony, by which we may understand that the paranchyma was found inflamed where the symptoms had been those of pleurisy; for such was the prejudice in favour of this division of pneumonia, that when it was found that the appearances on dissection did not correspond with it, it has been supposed that the one form of the disease had passed into the other, an opinion which seems to have been sanctioned even by Haller. Yet we find in some of the oldest writers more correct observations. Hippocrates speaks of pleurisy and peripneumony as affections of nearly, if not altogether, the same parts; and Galen observes that the pain in peripneumony is sometimes acute. Many observations to the same effect might be added from authors of equal authority, both with respect to the disease we are speaking of, and inflammatory affections of other organs.

will not always be found to be the case, for the capillaries sometimes suffer distension with little or no pain, particularly where the progress of the disease is slow. In general, however, in proportion as the distension is confined to the larger vessels there is less fever and less pain, and when they alone are affected there is little or none of either.

All local diseases producing fever, seem to consist in debility of the capillary vessels of the part affected. Dr. Cullen arranges them all under three heads, Inflammation, Hemorrhagy, and Profluvium. If we examine the symptoms of the two last we shall find, that except these diseases are of a mere passive nature, arising from external violence or extreme relaxation, in which cases they do not excite fever, their symptoms are those of inflammation relieved by discharges, in the one case, the effect of rupture of the vessels, in the other, apparently of distension of their extremities ; and it is particularly to be remarked, that it is only in proportion as the symptoms of inflammation prevail, that those of fever attend. It seems then from direct experiment to be a law of the animal economy, that debility and consequent distension of the capillary vessels, and this alone of all local affections, applies to the nervous system, such an irritation as excites to preternatural action the larger vessels of the part, and when of great extent or in vital parts, the whole sanguiferous system.

Do these observations throw any light on the nature of fever properly so called? In this disease we find a general debility of the capillaries followed by an increased action of the heart and larger vessels, its symptoms subsiding as soon as the capillaries are excited to the due performance of their functions. In such a state of the sanguiferous system, it is evident, that debilitating causes, acting partially, will readily increase the debility of the capillaries affected by them; and thus, as appears from what has been said, excite inflammation, which will either run its usual course or be relieved by hemorrhagy or profluvium. May we not thus account for the frequency of these affections in fever?

In this disease inflammation is particularly apt to arise in the brain, because the blood being returned thence, by membraneous canals, which cannot partake of the increased excitement of the central parts of the sanguiferous system *, this excitement necessarily tends to

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* The final cause of this structure appears to be to supply a greater temporary vigour on various occasions where the rapidity of the circulation is increased, particularly where great efforts are made, and consequently great excitement required in the muscles of voluntary motion; all organs being, within certain limits, more or less vigorous according to the quantity of blood circulating in them.

occasion accumulation of blood there. Inflammation is also apt to occur in fever in those parts where the vessels are most numerous and delicate, and where they are exposed to any species of injury; that is, where they are most apt to be debilitated.

According to this view of the subject fever must be regarded as a state of general inflammation, the symptoms peculiar to inflammation not appearing in any great degree, only because the increased *vis à tergo,* being so much smaller in proportion to the number of, and consequently the resistance opposed by, the debilitated vessels, than in inflammation where the vessels of only one part are debilitated, it cannot greatly distend them; and consequently, excite the more prominent symptoms of inflammation, unless they become particularly debilitated in some one part: but it often excites all these symptoms in a less degree, increase of heat, redness and fullness of the various surfaces. I may observe also, that in proportion as these symptoms appear the pulse becomes hard as in inflammation, and the buffy coat shews itself on the blood. Are not all the other symptoms of fever equally the consequences of this state of the circulation, the symptoms of excitement arising from the general effort of the sanguiferous system to excite the capillaries; those of debility from the state of the latter vessels, and the consequences of the ineffectual or but partially successful efforts to restore their due action? It is evident from what has been said, that the nervous is equally with the sanguiferous system engaged in these efforts.

In what may be called the local treatment of fever we find, that causes exciting the capillaries of any considerable part with which others sympathize, the sudden application of cold to the surface, the effect of cathartics in the alimentary canal, &c., tend to relieve this disease.

With respect to the general treatment, as the whole of the capillaries are debilitated, and the increased *vis à tergo* consequently bears a less proportion to the resisting force than in inflammation, it requires less reduction; but as even here it is apt to exceed the limits most favourable to the excitement of the capillaries, it must generally be reduced; and in proportion as we reduce it, we find, analogous to what happens in an inflamed part, that the redness, heat, and fulness of the various surfaces are relieved. It is equally necessary, however, both in fever and inflammation, to be careful that we do not so far reduce the *vis à tergo* that it can no longer support any degree of circulation in the debilitated capillaries; in which case we should have general sphacelus in simple fever, as we have local sphacelus in inflammation, were it possible that any of the functions of life could go on after all the capillaries had lost their power. In extreme cases of typhus we see a state approaching to this.

If the foregoing observations be correct, the treatment of fever is founded on the same principles with that of inflammation, the only difference being, that the local means are more extensively applied, in proportion as the local affection is more extensive, and as the resisting power is greater in fever than in inflammation, less vigorous means of reducing the *vis à tergo* are proper in the early stage of this disease, and in the latter stage, the means which support it are more frequently called for.

The attention of Mr. Knight, whose discoveries in the vegetable world have placed him in the first rank of philosophers, has been peculiarly attracted by the galvanic experiments which have been laid before the reader; and the strong analogy which subsists between animal and vegetable life, has induced him to reflect much on their results. He has favoured me with many ingenious suggestions relating chiefly to vegetable life, which will be submitted to the test of experiment. One relating to the subject before us, I cannot avoid mentioning, although I have not yet had an opportunity of attempting to profit by it. I mean the use of galvanism in the worst cases of typhus, in which there is an universal failure of the secreting power and the debility of the nervous system forms so prominent a feature. It may certainly be used with safety, and probably with advantage in this disease. The circumstance which appears to me to render it doubtful how far it may prove useful in typhus is, that here the due supply of fluids, as well as of nervous influence, fails. In restoring the former, galvanism can have no effect different from that of other stimuli. The proper mode of using it, I conceive to be, by many wires from one end of the trough applied to various parts of the head and spine, and many from the other end applied to such parts of the surface as shall send the influence through the body as much as possible in the direction of the nerves. Many of the observations which I shall have occasion to make on the use of galvanism in asthma, and which have been confirmed by repeated trials, will probably be found applicable to this and other cases in which it may be employed.

Having taken a cursory view of the nature of the diseases which arise from morbid distension, and consequent failure of circulation in the capillaries and larger vessels of the brain, we are now to consider the effects of a deranged state of this organ itself, and how far the experiments, which have been laid before the reader, throw light on the symptoms arising from this cause.

CHAP. III.

On Nervous Apoplexy.

A DIAGNOSTIC between sanguineous and nervous apoplexy, and the proper treatment of the latter, are still among the desiderata of medicine. The objects of the following observations are, to trace, as far as I can, with the aid of the experiments which have been laid before the reader, the distinguishing symptoms of nervous apoplexy ; to ascertain the circumstances which render the diagnostic between this disease and sanguineous apoplexy so difficult, a difficulty much to be lamented, as there is no instance in which a diagnostic is more necessary, these diseases often proving quickly fatal, and requiring very different treatment ; and lastly, to point out the principles on which the treatment of nervous apoplexy seems to be founded.

In considering the former parts of the subject, I shall, in the first place, point out what appears, from the principles which seem to be established by the experiments related in the preceding Inquiry, to be the necessary consequences of great injury of the brain; and then compare these consequences with the symptoms which actually attend diseased states of this organ in the human body.

As it appears, as far as I am capable of judging, from what has been said, that the leading features of sanguineous apoplexy depend on the fact, that the power of the heart and blood-vessels is independent of the nervous system, in consequence of which the power of the brain may be overwhelmed by a compressing force without directly affecting the powers of circulation*; so I think it will appear from what I am about to say, that the leading features of nervous apoplexy depend on the fact, that the power of the heart and blood-vessels, though independent of the nervous system, may be influenced even to its total destruction through this system†.

Let us consider the consequence of such an impression made on the nervous system as greatly lessens the power of the heart and blood-vessels. We have seen that agents acting on the brain and spinal marrow increase the action of the heart and blood-vessels, unless they are of a sedative quality, or applied in excess, that is, in such a degree as suddenly to injure the mechanism of the brain and spinal marrow, they then directly impair the powers of circulation*. If the mechanism of the brain be suddenly destroyed, instant death of all the functions ensues†. The cause applied, however, is rarely sufficient to produce this effect. It generally debilitates without destroying the various functions; the sensibility is impaired; the heart acts more frequently and feebly, and for the most part, irregularly; and the circulating system suffers a similar loss of power in every part of the body. The sphincters of the rectum and bladder do not merely cease to be excited by any voluntary effort as in sanguineous apoplexy, but have the power, on which the degree of contraction constituting their state of rest depends, more or less impaired. This state is succeeded by some improvement in the symptoms, the heart and blood-vessels in some degree recover from the shock they received‡. The former begins to beat with less frequency and with more force and regularity, and the latter to convey the blood with greater velocity and in a more uniform stream*. In proportion as this change takes place the various functions, as I have very frequently observed in rabbits, improve, the animal recovering a greater degree of sensibility. If the offending cause has been comparatively slight, the symptoms continue to improve; if severe, the heart soon begins again to beat more languidly, and with it all the functions gradually fail. If the injury done to the nervous system is of such a nature as particularly to debilitate the vessels of the injured part, during that interval, in which the vigour of the circulation is in some degree restored, the vessels of this part must yield to the *vis à tergo*, and the symptoms of inflammation are thus added to those arising from the original injury.

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* Exp. 18.

† Compare the Experiments related in Chap. I. of Part II., with Exp. 19, 20, 21, 22, 26, 27, 29, 30.

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* See the Experiments related in the second Chapter of Part II.

† Exp. 20, 21.

‡ Exp. 19, 29.

Such appears from the result of the experiments detailed in the preceding Inquiry, to be the consequences of such an injury of the brain and spinal marrow as materially deranges their mechanism. The reader will perceive that if the foregoing view of the subject be correct, the nervous is a much more complicated disease than the sanguineous apoplexy. In the latter, the powers of the nervous system are impaired, but those of the sanguiferous system are, in the commencement of the disease, entire, and only become affected through the failure of the functions of respiration and secretion. In nervous apoplexy, not only the powers of circulation suffer directly from the injury done to the nervous system, thus producing a combination of diseased states of both systems, but the debility of the heart and blood-vessels have a secondary effect on the nervous system. The action of the brain and spinal marrow fail from defective circulation, and a state of these organs, analogous to that which takes place in syncope, is superadded to that produced by the cause of the disease. It is not surprising, therefore, that this species of apoplexy sometimes proves instantly fatal; which sanguineous apoplexy, affecting the powers of circulation, only through the failure of other functions, cannot do, except it exists in such a degree as to produce instantaneous and total insensibility, which seldom if ever happens.

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* Exp. 19, 29.

The principles of the treatment in the former case also, are much more complicated. In sanguineous apoplexy, we have but one object in view, to relieve the brain from pressure. In nervous apoplexy, while we endeavour to counteract the effects of the offending cause on the brain, it is necessary to support the circulation; the failure of which, to a certain degree, must immediately prove fatal. This ought to be done, however, in such a way as tends least to occasion morbid distension of the vessels of the head, to which the cause of the disease often renders them liable; and which will produce either sanguineous apoplexy or phrenitis, according as the distension takes place in the larger or smaller vessels. From this view of the subject we may readily understand why abstraction of blood often proves fatal in nervous apoplexy, and yet much stimulus cannot be borne.

The simplest cases of nervous apoplexy, and those most nearly approaching to the state of the animals in the above experiments, are cases from mechanical injury of the brain. When a blow on the head fractures the skull, and occasions part of the bone to press on the brain without doing further injury to this organ, the case resembles in its nature the sanguineous apoplexy. When the compressing power is removed the apoplectic symptoms disappear; but when the blow has produced what surgeons call concussion of the brain, the case is only a slighter degree of the state in which the rabbits and frogs were found after the brain had been crushed.

No writer, perhaps, has detailed the symptoms of concussion of the brain with greater correctness than Mr. Abernethy, in the third part of his Surgical and Physiological Essays. It is impossible not to remark how accurately his account of these symptoms corresponds with the results of the experiments which have been laid before the reader:—" The whole train of " symptoms," he observes, " following a con- " cussion of the brain, may, I think, be pro- " perly divided into three stages. The first is, " that state of insensibility and derangement of " the bodily powers which immediately succeed " the accident. While it lasts the patient " scarcely feels any injury that may be inflicted " on him ; his breathing is difficult, but in ge- " neral without stertor, his pulse intermitting " and his extremities cold. But such a state " cannot last long ; it goes off gradually, and is " succeeded by another, which I consider as " the second stage of concussion. In this, the " pulse and respiration become better, and, " though not regularly performed, are suffi- " cient to maintain life, and to diffuse warmth " over the extreme parts of the body. The " feeling of the patient is now so far restored " that he is sensible if his skin is pricked, but " he lies stupid and inattentive to slight exter- " nal impressions. As the effects of concus- " sion diminish, he becomes capable of reply- " ing to questions put to him in a loud tone " of voice, especially when they refer to his " chief suffering at the time, as pain in the " head, &c. ; otherwise he answers incohe- " rently, and as if his attention was occupied

" by something else. As long as the stupor " remains, the inflammation of the brain seems " to be moderate, but as the former abates the " latter seldom fails to increase ; and this con" stitutes the third stage, which is the most " important of the series of effects proceeding " from concussion. These several stages vary " considerably in their degree and duration, " but more or less of each will be found to take " place in every instance where the brain has " been violently shaken." Page 59, 60. In the 67th page Mr. Abernethy observes—" It has " hitherto been considered as a desirable object " to point out any marks by which we might " distinguish between compression and con" cussion of the brain, but I believe no such " criteria have yet been communicated to the " public. I think, however, that these dis" eases may be distinguished. As far as my " observation goes, the insensibility is much " less in concussion, especially after a short " time has elapsed. Patients, in this case, " though they seem reluctant to answer ques" tions, yet complain much if their heads are " moved, and in those instances where it was " judged necessary to inspect the bone, I have " generally found that they made great com" plaint during the operation. The pupils also " are usually more contracted than in com" pression of the brain, the muscles of the

" limbs retain their natural state of tone, and " respiration is performed with little or no " stertor, though the pulse generally intermits " in a very considerable degree. In the " slighter cases of concussion the sickness of " the patient is often very great. But in cases " of compression of the brain circumstances " very much the reverse of those just related " take place. The sensibility is much diminished in proportion to the degree of the injury. From this cause also the pupils are " dilated and the limbs relaxed. The respiration is attended with stertor, and the " pulse, as far as my observation extends, is " subject to much less intermission." It is evident that in accidents we cannot always expect to find the symptoms of compression and concussion so distinct as in experiments made for the purpose of exhibiting those symptoms; many accidents tending at the same time to produce a greater or less degree of both affections.

The chief difference between the symptoms of concussion and nervous apoplexy arising from internal causes, is, that in the latter there is not so uniform a tendency to inflammation; which, in the cases referred to by Mr. Abernethy, is evidently the effect of the injury done to the vessels by the blow, which we have reason to believe causes them to suffer morbid distension as soon as a certain vigour of circulation is restored. It is this renewed vigour of circulation after the immediate effect of the blow has subsided, so remarkable in the experiments just referred to, that again gives some energy to the brain, and explains Mr. Abernethy's observation, that the stupor abates as the tendency to inflammation comes on.

In nervous apoplexy, from internal causes, the sensibility is often as much impaired as in the sanguineous apoplexy. When this is the case the danger is very urgent; but it frequently is much less so, compared with the severity of the other symptoms and the degree of danger; because here the sanguiferous, as well as the nervous system, suffers. In the former case, the derangement of function being confined to the nervous system, the danger is nearly proportioned to the degree of insensibility; but in the case before us, symptoms of the greatest danger often occur, although the patient is not wholly insensible, and not unfrequently while he is affected with a degree of irritability. The state of the pulse affords the the best diagnostic between these species of apoplexy. In the sanguineous, we have seen, it is strong, regular, and generally less frequent than natural; in the nervous, it is weak, frequent, irregular, and sometimes fluttering.

Such are the diagnostics of distinctly formed sanguineous and nervous apoplexy. Were these diseases always so formed, no attentive practitioner could be at a loss to distinguish them. But we have to lament that this is by no means the case, as indeed from what has been said we might *à priori* have supposed. For it must often happen in apoplexy, from distension of the vessels, that the brain will sustain some farther injury than that of mere uniform compression. It is not improbable that the circumstance of the compressing force, acting partially, may sometimes alone be sufficient to produce this effect; and powerful causes, injuring the mechanism of the brain, must often be of such a nature as at the same time to occasion debility, and consequently more or less distension of its vessels. To these circumstances, and to the difficulty of distinguishing apoplexy arising from mere distension of the vessels, from that arising from an extravasation of blood or serum, it appears to me that all the difficulties of the prognosis and diagnosis of the different species of this disease are to be ascribed.

It is the tendency to distension of the vessels of the encephalon that renders a very stimulating plan of treatment a doubtful practice, even in the most decided cases of nervous apoplexy. Were it not for this, the state of the sanguiferous and nervous systems in these cases would equally call for such a plan. But it would seem that the more debilitated the brain is, the more readily it feels the effects of any morbid distension of its vessels. Thus our practice in such cases is confined on all hands. Irreparable injury may be done by the free use either of stimuli or evacuants. The mode of treatment which has appeared to me the most successful, is a gently stimulating plan, combined for the purpose of preventing congestion of the head, with medicines moderately determining to the surface, and keeping the bowels free without occasioning a great discharge from them; with occasional abstractions of blood from the head, when the insensibility seems inclined to increase. It appears from what has been said, that the increase of this symptom is the best indication of the vessels of the head becoming more distended. Profuse sweating not relieving the symptoms, which is a frequent occurrence in severe cases of nervous apoplexy, seems always to indicate great danger, and to arise from a general relaxation of the extreme vessels. In cases arising from injuries of the head, Mr. Abernethy thinks the great tendency to inflammation altogether forbids the stimulating plan. I have already pointed out the circumstance which often makes the indications of cure in this respect different in concussion of the brain and nervous apoplexy arising from internal causes.

The foregoing view of the nature of the different species of apoplexy, not the result of preconceived opinions, but of facts open to the examination of every one who choses to repeat the experiments, and so strikingly confirmed by the observations of Mr. Abernethy and other writers on the effects of injuries of the brain, may tend, perhaps, to render the practice in this varied disease more determinate. It seems, by affording a more correct view of the *ratio symptomatum* of the sanguineous and nervous apoplexy, than could have been obtained without a knowledge of the relation which subsists between the sanguiferous and nervous systems, to point out with more precision than has yet been done, the symptoms essential to each, and consequently the modes of practice suited to the various cases in which they occur separately, or are blended together. I have entered no farther on these modes of practice than was necessary to point out the general principles on which they seem to be founded.

Inflammation of considerable extent, or of a vital part, we have seen, excites increased action of the sanguiferous system. The reader will more readily understand here, than he would have done in the Chapter on Inflammation, why in certain phlegmasiæ the action of this system, instead of being increased, is debilitated. I have just had occasion to observe, that distension of the vessels of the brain seems often, merely from the partial action of the distending power, so to injure this organ as to give rise to the symptoms of nervous apoplexy. A similar injury of the brain we might *à priori* suppose, must sometimes happen in that species of the distension of the vessels of the encephalon, which produces phrenitis; so that although in this disease the pulse is often strong, and the heat great, as in most other phlegmasiæ, it sometimes happens that the heat is but little increased, and the pulse small, frequent, and fluttering, the danger, for reasons just pointed out in speaking of the nature of nervous apoplexy, being very great. A similar state of the circulation is observed in other inflammations, which occasion very great nervous irritation. Thus in inflammation of the stomach and bowels, the heat is often little increased, and the pulse is feeble; the brain and spinal marrow being so injured as to weaken the action of the heart and blood-vessels, and thus cause a greater or less degree of syncope to be combined with the original disease. I have seen the powers of circulation so enfeebled by violent inflammation of the alimentary canal, that within twelve hours after the attack it was impossible to obtain four ounces of blood, although large veins in both arms and both legs, and one of the temporal arteries, were opened; no blood having been taken previously, and the patient, at the time of the attack, having been strong and in good health. He died in about twenty hours after the commencement of his disease. On inspecting the body, we found the whole of the alimentary canal inflamed, and a small spot on the stomach, of a purple colour, but no other morbid appearance. In such cases, however, the pulse, though feeble, is still hard, the general feebleness of the circulation not removing the necessity of supporting the motion of the blood in the more debilitated vessels of the inflamed part.

We see other causes of great nervous irritation, producing the same debility of the sanguiferous system. It appears from experiments above alluded to *, (an account of which was published in the second edition of my Treatise on Febrile Diseases), that throwing a strong solution of opium or tobacco into the cavity of the abdomen immediately enfeebles the power of the heart.

We readily understand from what has been said, why the above phlegmasiæ often and

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* Pages 133 and 134.

sometimes very suddenly prove fatal, without the inflammation running its usual course, the derangement of the nervous system being such as to destroy the powers of circulation. We also see why the pulse, in such cases, rises after blood-letting, which lessens the offending cause. I believe that in some other cases in which the pulse rises after blood-letting, this effect may be explained in the same way. On the same principle also, I think, we must explain the sudden debility, and subsequent loss of power in the circulating system, which ensues on gangrene of any of the vital organs.

CHAP. IV.

On Affections of the Spinal Marrow.

The experiments in which different portions of the spinal marrow were destroyed* seem to throw considerable light on the nature of the symptoms occasioned by diseases of this organ. We have seen that the destruction of any part of it not only renders paralytic, that is, deprives of their only stimulus, the muscles of voluntary motion which correspond to that part, and to all parts of the spinal marrow lying below it*; but, by lessening the supply of nervous influence to the great chain of ganglions, influences the state of the thoracic and abdominal viscera and the temperature of the animal. In estimating the effect on the thoracic and abdominal viscera of destroying portions of the spinal marrow in animals, we must trust rather to the appearances observed in the lungs and stomach after death than to the symptoms produced; because as the animal can give no account of its feelings, no symptom of deranged digestion appears till it goes so far as to produce efforts to vomit, nor of oppressed breathing till it goes so far as to produce evident dyspnœa. Thus in the experiments just referred to, it appeared on dissection, that the process of digestion was sometimes wholly suspended, and the lungs more or less congested, where no symptoms of indigestion, and little or no change in the breathing, had been observed. We often complain of affections of the stomach and lungs long before their symptoms can be perceived by others. Congestion and even inflammation of the lungs do not excite cough in the rabbit.

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* Exp. 54, 55, 56.

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* It appears from what has been said, that although both the muscles corresponding to the part of the spinal marrow destroyed, and those corresponding to all parts below it, equally cease to move, it is from different causes; the former, because their nervous influence is destroyed; the latter, because their nervous influence is no longer subject to the sensorium. Whether in the former case the power of the muscles themselves is impaired, will depend on the rapidity with which the offending cause has operated. See Part II., Chap. II.

Even in early stages of diseased spine, affections of the stomach and lungs frequently attend, and the patient often complains of a sense of cold. Mr. Pott remarks of this disease, " loss of appetite, a hard dry cough, labo-" rious respiration, &c., appear pretty early, " and in such a manner as to demand atten-" tion." And in another place he observes, that there is " an unusual sense of coldness of " the thighs, not accountable for from the " weather." Similar observations are made by every writer on diseased spine. How well they correspond with the foregoing views need not be pointed out. From what I shall have occasion to say of asthma and dyspepsia the reader will see reason to believe, that the foregoing symptoms may probably be relieved from time to time by the use of galvanism.

It appears from experiment 59, in which the spinal marrow was simply divided, compared with experiments 54, 55, 56, in which portions of it were destroyed, that we may judge of the extent of the injury done to this organ, in diseases of the spine, by the state of the stomach and lungs. Any thing, which so affects the spinal marrow as to interrupt the communication between the brain and other parts, will of course prevent the influence of the will from reaching them, however small a part of the spinal marrow may be injured. But if a considerable part of it is so, along with loss of power in the limbs, the patient will experience symptoms of dyspepsia and oppressed breathing proportioned to the importance and extent of the part whose function is destroyed. I have already had occasion to explain why the lungs are particularly affected by the destruction of the dorsal, and the intestines by that of the lumbar portion of the spinal marrow *.

The experiments related in the preceding inquiry seem to point out more precisely than former observations have done, what we are to expect from the use of galvanism in the cure of disease; and I think it will appear from what I am about to say, that to the want of discrimination in its employment we must ascribe the little advantage which medicine has hitherto derived from the discovery of this influence †.

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* See Chap. IX.

† Many of the following observations on Galvanism are re-published from a paper which the Royal Society did me

It is an inference, I have already had occasion to observe, from my own experiments and observations*, as well as those of others, particularly of M. le Gallois, that what is called the nervous system, comprehends two distinct systems, the sensorial, and the nervous system properly so called. Now, it does not appear from the foregoing Inquiry, that galvanism can perform any of the functions of the sensorial system, yet, in the greater number of instances in which it has been used in medicine, it has been expected to restore the sensorial power. It has been expected to restore hearing, and sight, and voluntary power. It may now and then happen in favourable cases, from the connexion which subsists between the sensorial and nervous systems, that by rousing the energy of the latter, we may excite the former. It would be easy to show, that we have little reason to expect that this will often happen. We have also reason to believe, from the experiments which have been laid before the reader, that galvanism has no other power over the muscular system than that of a stimulus†; we

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the honour of publishing in the Philosophical Transactions of last year.

* See Chap. X., and the experiments there referred to.

† Compare the experiments related in the first and second Chapters of this part of the Inquiry with Exp. 70, 71, 72, 73, and the observations which follow them.

are, therefore, to expect little more advantage from it in diseases depending chiefly on faults of the sanguiferous system, than from other stimuli, &c. But I cannot help regarding it as almost ascertained, that in those diseases in which the derangement is in the nervous power alone, where the sensorial functions are entire, and the vessels healthy, and merely the power of secretion, which seems immediately to depend on the nervous system, is in fault, galvanism will often prove a valuable means of relief.

CHAP. V.

On Asthma and Dyspepsia.

As soon as the foregoing view of the subject presented itself, I was led to inquire, what diseases depend on a failure of nervous influence. The effect on the stomach and lungs, of dividing the eighth pair of nerves*, answered the question respecting two of the most important diseases of this class. We have seen, that withdrawing a considerable part of the nervous influence from the stomach and lungs deranges the digestive powers, and produces great difficulty of breathing. The following observations relate chiefly to affections of the lungs. Of the effects of galvanism in dyspepsia, the principal experience which I have yet had, has been in cases where it was complicated with asthmatic breathing.

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* Exp. 44, 45.

When the effect of depriving the lungs of a considerable part of their nervous influence is carefully attended to, it will be found, I think, in all respects similar to a common disease, which may be called habitual asthma; in which the breathing is constantly oppressed, better and worse at different times, but never free, and often continues to get worse in defiance of every means we can employ, till the patient is permanently unfitted for all the active duties of life. The animal, in the above experiment, is not affected with the croaking noise and violent agitation which generally characterize fits of spasmodic asthma. This state we cannot induce artificially, except by means which lessen the aperture of the glottis*.

We have seen from repeated trials, that both the oppressed breathing and the collection of phlegm, caused by the division of the eighth pair of nerves, may be prevented by sending a stream of galvanism through the lungs†. That this may be done with safety in the human body we know from numberless instances, in which galvanism has been applied to it in every possible way.

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* See Chap. V.

† Exp. 70, 71, 72, 73.

Such are the circumstances which led me to expect relief from galvanism in habitual asthma. It is because that expectation has not been disappointed, that I trouble the reader with the following account of its effects. Although the effects of galvanism in habitual asthma have been witnessed by many other medical men, I have mentioned nothing in the following pages which did not come under my own observation.

I have employed galvanism in many cases of habitual asthma, and almost uniformly with relief. The time, during which the galvanism was applied before the patient said that his breathing was easy, has varied from five minutes to a quarter of an hour. I speak of its application in as great a degree as the patient could bear without complaint. For this effect I generally found from eight to sixteen four-inch plates of zinc and copper, the fluid employed being one part of muriatic acid, and twenty of water, sufficient. Some require more than sixteen plates, and a few cannot bear so many as eight; for the sensibility of different individuals to galvanism is very different. It is curious and not easily accounted for, that a considerable power, that perhaps of twenty-five or thirty plates, is often necessary on first applying the galvanism, in order to excite any sensation; yet after the sensation is once excited, the patient shall not perhaps, particularly at first, be able to bear more than six or eight plates. The stronger the sensation excited, the more speedy in general is the relief. I have known the breathing instantly relieved by a very strong power. I have generally made it a rule to begin with a very weak one, increasing it gradually at the patient's request, by moving one of the wires from one division of the trough to another, and moving it back again when he complained of the sensation being too strong. It is convenient for this purpose to charge with the fluid about thirty plates.

The galvanism was applied in the following manner. Two thin plates of metal, about two or three inches in diameter, dipped in water, were applied, one to the nape of the neck, the other to the pit of the stomach, or rather lower. The wires from the different ends of the trough *

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* I found a trough, of the old construction, more effectual in restoring the due action of the lungs than the improved pile. I was at first at a loss to account for this circumstance. From many observations I have now reason to believe, that it arises from such effects of galvanism, like its other effects on the animal body, being proportioned, not to the quantity of electricity

were brought into contact with these plates, and, as observed above, as great a galvanic power maintained, as the patient could bear without complaint. In this way the galvanic influence was sent through the lungs, as much as possible, in the direction of their nerves. It is proper, constantly to move the wires upon the metal plates, particularly the negative wire, otherwise the cuticle is injured in the places on which they rest. The relief seemed much the same, whether the positive wire was applied to the nape of the neck, or the pit of the stomach. The negative wire generally excites the strongest sensation. Some patients thought, that the re-

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supplied by the trough, but to the intensity of its electrical and quantity of its chemical power, both of which are proportioned rather to the number of plates, than to the extent of surface.

I have repeatedly tried the effects of a powerful electrical machine, in habitual asthma. They are considerable, but inferior to those of the voltaic trough; which I would ascribe to the former possessing much less chemical power, in proportion to the intensity of its electricity, than the latter. The most powerful electrical battery will not readily decompose water without the ingenious arrangement, suggested by Dr. Wollaston, for concentrating, as much as possible, its electrical power, while the power of a few voltaic plates is, without any precaution, sufficient for this purpose.

There is reason to believe that plates of an inch and half, or two inches, square, would answer medical purposes nearly as well as larger plates.

lief was most speedy, when it was applied near the pit of the stomach.

The galvanism was discontinued as soon as the patient said that his breathing was easy. In the first cases in which I used it, I sometimes prolonged its application for a quarter of an hour, or twenty minutes, after the patient said he was perfectly relieved, in the hope of preventing the early recurrence of the dyspnœa; but I did not find that it had this effect. It is remarkable, that in several who had laboured under asthmatic breathing for from ten to twenty years, it gave relief quite as readily as in more recent cases; which proves, that the habitual difficulty of breathing, even in the most protracted cases, is not to be ascribed to any permanent change having taken place in the lungs.

With regard to that form of asthma which returns in violent paroxysms, with intervals of perfectly free breathing, I should expect little advantage from galvanism in it, because, as I have just observed, I found that the peculiar difficulty of breathing, which occurs in this species of asthma, cannot be induced in animals, except by means lessening the aperture of the glottis. It is probable, that in the human subject the cause producing this effect is spasm, from which, indeed, the disease takes its name, and we have no reason to believe, from what we know of the nature of galvanism, that it will prove the means of relaxing spasm.

The spasmodic asthma is fortunately a very rare disease, so much so, that but two cases of it have occurred to me since I have employed galvanism in asthma, while I have had an opportunity of employing this remedy in about fifty cases of the habitual form of the disease. I cannot, therefore, from experience, speak with certainty of the effect of galvanism in the former. In the above cases it was employed in the paroxysm, and I could observe no relief from it. In one of them, the patient said, that, had it not been used, the symptoms would have been more severe. In this patient, the spasmodic paroxysm was often succeeded by a state of habitual asthma for several weeks, in which galvanism gave immediate, but temporary relief.

Of the above cases of habitual asthma, many occurred in work-people of the city where I reside, who had been obliged to abandon their employments in consequence of it, and some of them, from its long continuance, without any hope of returning to regular work. Most of them had tried the usual means in vain. By the use of galvanism they were relieved in different degrees, but all sufficiently to be restored to their employments. I have seen several of them lately, who, although they have not used galvanism for many months, said they had continued to work without any inconvenience. Some, in whom the disease had been wholly removed, remain quite free from it; some have had a return of it, and have derived the same advantage from galvanism as at first.

I have confined the application of galvanism to asthmatic dyspnœa. I think there is reason to believe, from the experiments which have been laid before the reader, that in inflammatory cases it would be injurious; and, in cases arising from dropsy, or any other mechanical impediment, little or nothing, it is evident, is to be expected from it. Habitual asthma is often attended with a languid state of the biliary system, and some fullness and tenderness on pressure near the pit of the stomach. If the last is considerable, it must be relieved previous to the use of the galvanism. In a paper which the Medico-Chirurgical Society did me the honour to publish in the seventh volume of their Transactions, I have endeavoured to shew that a species of pulmonary consumption arises from a disease of the digestive organs. Some of the observations there made apply to certain cases of asthma *; I believe, to cases of every

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* See also the observations on the state of these organs in asthma, in Dr. Bree's work on this disease.

species of this disease, but particularly of that we are here considering. Many cases of habitual asthma will yield to the means recommended in the above paper, but I have learned, from a pretty extensive experience, that a large majority of such cases will resist them, yet readily admit of relief from galvanism. If there is little tendency to inflammation, galvanism seems also to be a means of relieving the affection of the digestive organs. I have repeatedly seen from it the same effect on the biliary system which arises from calomel; a copious bilious discharge from the bowels coming on within a few hours after its employment. This seldom happens except where there appears to have been a failure in the secreting power of the liver, or a defective action in the gall tubes.

I have not found that the presence even of a severe cough, which is common in habitual asthma, in which there is always more or less cough, counter-indicates the use of galvanism. The cough, under its use, generally becomes less frequent in proportion as the accumulation of phlegm in the lungs is prevented; but it seems to have no direct effect in allaying it. During the application of the galvanism the patient is sometimes excited to cough up the phlegm, which is oppressing the lungs. It frequently, however, disappears without being coughed up.

In some cases the cough continued troublesome after the dyspnœa had disappeared. Galvanism never appeared to increase it, except when the inflammatory diathesis was considerable. In some labouring under the most chronic forms of phthisis, in whom the symptoms had lasted several years and habitual asthma had supervened, the relief obtained from galvanism was very great, notwithstanding some admixture of a pus-like substance in what was expectorated. I need hardly add, after what has been said, that in ordinary cases of phthisis nothing could be more improper than the use of galvanism. The dyspnœa arising from phthisis and that from habitual asthma are easily distinguished. The former is less variable. It is generally increased by the exacerbations of the fever, and always by exercise. When the patient is still and cool, except in the last stages of phthisis, his breathing is generally pretty easy. The latter is worst at particular times of the day, and frequently becomes better and worse without any evident cause. At the times when it is better the patient can often use exercise without materially increasing it., It is unaccompanied by fever. Changes of the weather influence it much. It is particularly apt to be increased by close and foggy weather. Phthisical dyspnœa is seldom much influenced by changes of the weather.

except they increase the inflammatory tendency.

When there is a considerable tendency to inflammation in habitual asthma, the repeated application of galvanism sometimes increases it so much, that the use of this influence no longer gives relief, till the inflammatory tendency is subdued by local blood-letting. It always gave relief most readily, and the relief was generally most permanent in those cases which were most free from inflammatory tendency, and least complicated with other diseases, the chief complaint being a sense of tightness across the region of the stomach, impeding the breathing. The patients said, that the sense of tightness gradually abated while they were under the influence of the galvanism, and that as this happened their breathing became free. The abatement of the tightness was often attended with a sense of warmth in the stomach, which seemed to come in its place. This sensation was most frequently felt when the negative wire was applied near the pit of the stomach, but the relief did not seem less when it was not felt.

With respect to the continuance of the relief obtained by galvanism, it was different in different cases; in the most severe cases it did not last so long as in those where the symptoms were slighter, though of equal continuance. This observation, however, did not universally apply. When the patient was galvanised in the morning, he generally felt its good effects more or less till next morning. In almost all, the repetition of the galvanism gradually increased the degree of permanent relief. Its increase was much more rapid in some cases than in others. The permanency of the good effects of galvanism in the disease before us, has appeared very remarkably in several cases where the symptoms, after having been removed by it, were renewed after intervals of different duration by cold or other causes. In these cases means which, previous to the use of galvanism, had failed to give relief, were now successful without its aid ; or with few applications of it, compared with those which had been necessary in the first instance. I have not yet seen any case, in which galvanism had been of considerable advantage, where its good effects appeared to have been wholly lost. It is now about two years and a half since I first employed it in habitual asthma. Taking cold and the excessive use of fermented liquors have been the principal causes of relapse.

The galvanism was seldom used more than once a day. In some of the more severe cases it was used morning and evening. About a sixth part of those who have used it appear, as far as we yet know, to have obtained a radical cure. It in no case failed to give more or less relief, provided there was little inflammatory tendency. It failed to give considerable relief only in about one-tenth; I may add, that were it only the means of present relief, we have reason to believe that, as being more innocent, it would be found preferable to the heating, spirituous, and soporific medicines, which are so constantly employed in this disease.

As it often happened that a very small galvanic power, that of not more than from four to six four-inch double plates, relieved the dyspnœa, may we not hope, that a galvanic apparatus may be constructed, which can be worn by the patient, of sufficient power to prevent its recurrence in some of the cases in which the occasional use of the remedy does not produce a radical cure?

I wished to try, if the impression on the mind, in the employment of galvanism, has any share in the relief obtained from it. I had not at this time seen its effects in apoplexy. I found that by scratching the skin with the sharp end of a wire, I could produce a sensation so similar to that excited by galvanism, that those who had most frequently been subjected to this influence were deceived by it. By this method, and arranging the trough, pieces of metal, &c., as usual, I deceived several who had formerly received relief from galvanism, and also several who had not yet used it. All of them said that they experienced no relief from what I did. Without allowing them to rise, I substituted for this process the real application of galvanism, merely by immersing in the trough without their knowledge, one end of the wire with which I had scratched the nape of the neck, the wire at the pit of the stomach having been all the time applied as usual by the patients themselves. Before the application of the galvanism had been continued as long as the previous process, they all said they were relieved. I relate the particulars of the two following experiments, because, independently of the principal object in view in making them, they point out two circumstances of importance in judging of the *modus operandi* of galvanism in asthmatic cases.

The first was made on an unusually intelligent lady, of about thirty-five years of age, who had for many years laboured under habitual asthma, than whom I have known none more capable of giving a distinct account of their feelings. Her breathing was very much oppressed at the time that she first used galvanism. The immediate effect was, that she breathed with ease. She said she had not breathed so well for many years. Part of the relief she obtained proved permanent, and when she was galvanised once a day for about ten minutes, she suffered little dyspnœa at any time. After she had been galvanised for eight or ten days, I deceived her in the manner just mentioned. The deception was complete. She told me to increase or lessen the force of the galvanism, as she was accustomed to do, according to the sensation it produced. I obeyed her directions by increasing or lessening the force with which I scratched the neck with the wire. After I had done this for five minutes, she said the galvanism did not relieve her as usual, and that she felt the state of her breathing the same as when the operation was begun. I then allowed the galvanism to pass through the chest, but only through the upper part of it, the wire in front being applied about the middle of the sternum. She soon said that she felt a little relief; but although it was continued in this way for ten minutes, the relief was imperfect. I then directed her to apply the wire in front to the pit of the stomach, so that the galvanism passed through the whole extent of the chest, and, in a minute and a half, she said her breathing was easy, and that she now experienced the whole of the effect of the former applications of the remedy.

To try how far the effect of galvanism in asthma arises merely from its stimulating the spinal marrow, in a young woman who had been several times galvanised in the usual way, the wires were applied to the nape of the neck and small of the back, and thus the galvanic influence was sent along the spine for nearly a quarter of an hour. She said her breathing was easier, but not so much so as on the former applications of the galvanism; and on attempting to walk up stairs she began to pant, and found her breathing, when she had gone about half way, as difficult as before the galvanism was applied. She was then galvanised in the usual way for five minutes: she now said her breathing was quite easy, and she walked up the whole of the stairs without bringing on any degree of panting, or feeling any dyspnœa. The above experiment was made in the presence of four medical gentlemen. This patient, after remaining free from her disease for about half a year, returned to the Infirmary, labouring under a slighter degree of it, and experienced immediate relief from galvanism. The disease seemed to have been renewed by cold, which had at the same time produced other complaints. This is one of the cases above alluded to in speaking of the permanency of the good effects of galvanism. On the return of this patient to the Infirmary, two or three applications of galvanism, combined with means which had given no permanent relief to the dyspnœa previous to her first using galvanism, now soon removed it. When she first used galvanism, it required its constant employment once or twice a day for several weeks to produce the same effect. There is reason to believe she will remain well if she can avoid taking severe colds.

Many medical gentlemen have frequently witnessed the relief afforded by galvanism in habitual asthma, and Mr. Cole, the house surgeon of the Worcester Infirmary, authorizes me to say, that no other means there employed have been equally efficacious in relieving this disease.

Observations similar to the foregoing, there is reason to believe, will be found to apply to dyspepsia; but, as I observed above, having made but few trials of galvanism in this disease, except where it was complicated with asthma, the removal of which no doubt contributed to a more healthy action of the digestive organs, I cannot yet speak with certainty of its effects in this disease. In some, galvanism, at the time of its application, occasions a tendency to sighing; and in some, in whom it removed the dyspnœa, it seemed to occasion a sense of sinking referred to the pit of the stomach. This occurred in several instances, and was relieved by small doses of carbonate of iron and bitters.

That I may convey to the reader as correct an idea as I can of the effects of galvanism in habitual asthma, I shall concisely relate the particulars of a few of the most, and of the least, successful cases, in which it was employed.

Richard Morgan, a blacksmith, æt. 50, had laboured under severe habitual asthma for seven months, during which he had been better and worse for a few weeks, but never free from dyspnœa. He was much troubled with a cough, the expectorated matter being thick, and of a yellowish colour. The dyspnœa was particularly severe at the time he was galvanised, and had been so for about a fortnight. The first application of the galvanism relieved him. He was galvanised only for three days, about ten minutes each day, before he declared himself to be perfectly well. He returned to his work, which he had been obliged to abandon, after the second application of the galvanism. After its third application he performed as hard work, and with as much ease, as he had ever done.

He remained free from dyspnœa till it was renewed, several weeks afterwards, by intoxication. Galvanism relieved him as readily and effectually as at first. It is now ten months since he first used this remedy, during which he has had several returns of dyspnœa, but it has never been so severe as before he was galvanised; and when it has been such as to induce him to have recourse to galvanism, he has always experienced from it immediate relief. He ascribes the returns of his disease to his being exposed to severe and sudden heats and chills.

Mary M'Konchy, æt. 28, a gloveress, had been afflicted with habitual asthma for four years, and under my care about one year, during which she had tried all the usual means with very imperfect relief; she had some languor in the biliary system, but little inflammatory tendency. The breathing was, in a few minutes, rendered easy by galvanism, and after the second application of it, it remained so. She now experienced no inconvenience from exercise, which had not at any time been the case for four years.

In about three weeks after she had been galvanised she experienced some return of the dyspnœa. It was wholly removed by a blister, which had often been tried, previous to her being galvanised, with but little and very temporary relief. She complained of a sense of sinking at the stomach for some time after the use of the galvanism, which was removed by carbonate of iron and bitters. This effect of galvanism seemed often to be most felt when it gave most relief to the dyspnœa, seeming to come in place of the latter. I have hitherto found it easily removed by the above means. It is now many months since this patient was galvanised, and she remains well.

Hannah Cooke, æt. 20, a servant, had laboured under habitual asthma for two months, and tried various medicines without relief. She was in a few minutes relieved by galvanism, and after three applications of it remained quite well. It is now five or six weeks since she was galvanised.

I could mention several other cases, in which I witnessed the same sudden and permanent relief from galvanism, as in those here related.

Isaac Radley, æt. 68, a labourer, formerly a soldier, had been ill fourteen years. His asthma was caused by sleeping in camp in Holland. He had never been able, during the above time, to walk at the usual pace without bringing on the dyspnœa, although he had sometimes been pretty free from it when he was still; at other times he had been constantly oppressed with it, and obliged wholly to abandon his work. At the time he used the galvanism, he was affected with the most severe dyspnœa, which only allowed him to move, and that with difficulty, at the slowest pace; he had been in this state for half a year. This was the longest and most severe fit he had ever had. He was relieved in a few minutes by the application of galvanism. He could perceive its beneficial effects for twenty-four hours after its application. It was used daily with the same immediate relief. Its permanent good effects gradually increased, and after he had been galvanised for about ten minutes each day, for between two and three weeks, his breathing remained quite easy. He could now not only walk, but, as I several times witnessed, run without any dyspnœa. He complained of the sense of sinking at the pit of the stomach after the dyspnœa had left him, which, as in the case just mentioned, was readily removed by the carbonate of iron and bitters. He now said his digestion was much better than it had been previously to the use of the galvanism. Those whose breathing had been much relieved by galvanism, often made this observation, although they had not experienced the sense of sinking, and consequently had used no stomachic medicines.

I saw this man, several months after he had ceased to use galvanism, working as a bricklayer's labourer. He said he had no feeling of dyspnœa. It is now above two years since he used the galvanism, and he has had no return of the disease.

In general, where galvanism gave such complete and permanent relief, as in Radley's case, its effects were more speedy, some degree of dyspnœa for the most part remaining in protracted cases.

The following are the most unsuccessful cases, which either Mr. Cole or I could recollect.

Martha Davies, a servant, æt. 40, had laboured under habitual asthma for five years. She was relieved on the first application of galvanism, and said her breathing was quite easy; but she was not always equally relieved by it, sometimes it gave comparatively little relief. The more permanent relief afforded, was also different at different times, never complete. She was galvanised for about three weeks, but not daily, her business preventing her regular attendance; she used the remedy in all thirteen or fourteen times. It was impossible to prevent her drinking a great deal too much malt liquor.

It is now about half a year since she was galvanised, during which she says both her breathing and digestion have been better than for the preceding five years. She thinks the digestion as much improved as the breathing. She has had no very bad attacks of dyspnœa, and has been much less subject to bilious attacks. She is now occasionally so well that she can run without inconvenience, which she could never do during the above time; but, in general, her breathing, though in a less degree than formerly, is still oppressed.

Mary Clark, a servant, æt. 24, had laboured under habitual asthma for about a year. The dyspnœa was always quickly relieved by the galvanism, although she seemed to experience little, if any, permanent relief from it. She had more pain in the stomach than is usual in such cases, and the galvanism seemed to increase it. She was cured by an alterative course of medicines and evacuations from the region of the stomach, and did not use galvanism for the last fortnight. She had used it at first daily for a fortnight, and twice afterwards for a week each time.

As far as I can judge from having observed the course of many cases of this kind, her recovery would neither have been so speedy nor complete if she had not used galvanism.

Rachel Hooper, æt. 29, a servant, had laboured under severe habitual asthma for about a year, with considerable inflammatory tendency. Her breathing was relieved in a few minutes by galvanism, but not completely. For about eight or ten days, during which she was galvanised daily for about ten minutes, she derived from it considerable relief, both immediate and permanent. It then began to fail to give relief, and in a few days gave none. The epigastric region was now very tender on pressure. This symptom was relieved in the space of a few days by local blood-letting, blistering, and small doses of calomel. The relief afforded by the galvanism was now greater than at first, which seemed to arise from the disease not being so severe as on the first use of the remedy, for some part of the good effects of the galvanism had remained. After this she was always relieved by it as long as she continued to use it, which was for several weeks. The permanent relief she experienced from it was also great, although she still at times laboured under a considerable degree of dyspnœa. About a year and half ago, she left Worcester, with a promise to return, if she should get worse. I have heard nothing of her since.

She said nothing else had given her so much, either immediate or permanent, relief, as the galvanism had done. She had been for several months in the infirmary under other plans of treatment before she used the galvanism. All the patients whose cases I have mentioned were galvanised at the infirmary.

The following is a remarkable instance of permanent, though imperfect, relief, from galvanism, in the disease before us. A woman, who had for many years laboured under severe habitual asthma, was incautiously galvanised with such a power as occasioned severe pain. No intreaty could induce her to submit to a repetition of the galvanism, although it had immediately relieved her breathing. The dyspnœa soon recurred, but she told me many months afterwards that it had never been so severe since she was galvanised, and that she had ever since been able to carry water in buckets from the river, which the state of her breathing had not for a long time previously allowed her to do.

If the reader will compare these cases with the general observations which I have had occasion to make on the effects of galvanism in habitual asthma, he will be enabled to form a pretty correct estimate of what he may expect from its employment in this disease.

When we compare them with the experiments laid before the reader in the preceding Inquiry, the question naturally arises, Whence proceeds the permanent relief obtained in them? The galvanic experiments lead us to expect relief to the dyspnœa while the stream of galvanism passes through the lungs; but on what principle shall we explain the permanency of the relief afforded? The following observations appear to throw some light on this subject. There are two ways in which an organ may be deprived of its nervous influence, either by a failure of due action in the brain and spinal marrow, the sources of nervous influence, or a failure of due action in the nerves of the organ affected by which this influence is conveyed. It is no longer conveyed by a nerve which has been divided, or around which a ligature has been thrown. Now we have reason to believe, that habitual asthma arises not so much from a fault in the brain and spinal marrow, as in the nerves of the lungs; because, did the degree of dyspnœa, which we often witness in this disease, arise from failure in the general source of nervous influence, this failure must be sufficient to appear in the derangement of all the nervous functions; whereas in habitual asthma, we often find the function of the lungs alone affected; and when general failure of nervous influence is observed, it is evidently the effect of impeded respiration, appearing only after the latter has continued for some time, and varying as it varies. The effect produced by galvanism, when it performs a cure in habitual asthma, therefore, does not appear to be its having occasioned a permanent supply of nervous influence, but its having cleared, if I may use the expression, the passage of this influence to the lungs. It is not difficult to conceive that such an obstruction may exist in the nerves as cannot be overcome by the usual supply of nervous influence, though it may yield to a greatly increased supply of it; and that it may in some cases continually recur in an equal or diminished degree, while in others, being once removed, the tendency to it may cease. What is here said is well illustrated by the effects of galvanism in apoplexy. We know that in this disease the dyspnœa arises from a failure in the source of nervous influence, and the relief obtained from galvanism corresponds with the views afforded by the experiments which have been laid before the reader. While the galvanism passed through the lungs the dyspnœa was as much relieved as in habitual asthma, but when it ceased to pass through them, the relief lasted no longer than was necessary for the re-accumulation of the phlegm.

The foregoing observations seem to explain why other means, which give a temporary vigour to the nervous system, often, for the time, relieve habitual asthma; and sometimes, though rarely, cure this disease. The relief obtained from such means being in general so much less than that obtained from galvanism, I would ascribe to the former occasioning but little additional supply of nervous influence, while by the latter we can make the additional supply as great as we please.

CHAP. VI.

On Suspended Animation.

The last disease which I shall mention is suspended animation from drowning, or other causes obstructing the breathing. Inflating the lungs seems here to act in two ways. It gives to the blood of the smaller vessels of the lungs the arterial properties by which they are excited to action; and acting through the blood of these vessels, it communicates to that of the larger vessels, and of the heart itself, more or less of the same properties, independently of the blood already changed being moved on towards this organ; for M. le Gallois has shewn, that after the circulation has permanently ceased, the blood may be changed, by inflating the lungs, not only in the trunks of the pulmonary veins and the heart itself, but also in the aorta. By these means the circulation in the lungs is restored, but it is evident from the experiments which have been laid before the reader, that their due action cannot be restored till they receive their usual supply of nervous influence. Now this cannot happen till the re-established circulation has renewed the vigour of the brain and spinal marrow. We have reason to believe, that could the due degree of this influence be restored to the lungs as soon as the circulation is renewed in them by the access of the air, they would be excited to a more perfect performance of their functions; and that recovery might thus be effected in some cases, where inflation of the lungs alone would fail.

We have seen, from direct experiment, that galvanism can supply the place of nervous influence in the lungs, enabling them to perform their functions after the latter is withdrawn. I would, therefore, propose, that, to the means employed for the recovery of suffocated persons, an apparatus, properly adapted for sending a stream of galvanism through the lungs in the direction of their nerves, as above pointed out, should be added. It would be improper here to employ, for any considerable length of time, a stronger power than experience has taught us can be used without bad effects in health. The power should not exceed that of fifteen, or at most, twenty, four-inch double plates of zinc and copper, the fluid being one part of muriatic acid and twenty of water.

I should expect little advantage from galvanism applied to any other secreting organ, because the revival of the patient depends little, if at all, on the action of any other. Employed as a general stimulus to the brain and spinal marrow, it may be of use by rousing the dormant powers of the system. They are all, we have seen, capable of being excited through these organs. In this way it can only indirectly assist the lungs, and that chiefly in proportion to the degree in which general circulation is restored. It is probable, that as a general stimulus, a greater power of galvanism may be used without injury, than it would be proper to send through any vital organ for a considerable length of time, because employed with this view, it may be applied interruptedly.

Do not the experiments which have been laid before the reader tend to throw some light on the nature of the sympathy which exists between different parts of the body, and so extensively influences the symptoms and treatment of diseases*? If it appears that the nervous influence is not only capable of exciting, and acting as a sedative to, the moving fibre where-ever it exists, and whether subject to the will or not†, and of influencing, in every possible way, the secreting process‡, but is itself of such a nature, that it is capable of pervading equally the solids and fluids of the body, and of being instantly moved from place to place, through the medium of the central parts of the nervous system, independently of any immediate connexion of vessels or nerves§, it will not be difficult to explain the various phenomena of sympathy, many of which at first view appear so unaccountable.

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[*] I have, in a paper above referred to, published in the seventh volume of the Medico-Chirurgical Transactions, enumerated some of the more striking instances in which the sympathy of parts influences the symptoms and treatment of diseases.

[†] Part II. Chap. 2. [‡] Chap. 5.

[§] Chap. 5. The phenomena of sympathy seem always to take place through the intervention of the brain or spinal marrow. We still, by sympathy, refer sensations to parts, which are lost, till new associations are formed. The immediate cause of sensation in these cases must exist in the sensorium. We cannot shew why it should so exist in these cases and not in all.

APPENDIX.

I HAVE in the Preface given my reasons for entering on the subject of this Appendix. They will more fully appear in the circumstances which I am about to lay before the Reader. So much has lately been said, particularly by some members of the Royal Society, respecting the cruelty of experiments on living animals, and so much of what has been said personally applied to me, that I cannot help feeling myself called upon to vindicate my character against charges, which, I hope, I shall easily make it appear, have no foundation except in mistake respecting facts, and the most narrow views in reasoning on them.

By some I have been censured for the number of my experiments on living animals; by others for the minuteness with which I have sometimes detailed them; some object to making any such experiments at all, others, I understand, admitting their occasional necessity, have objected to their being laid before the Royal Society, there being in this country societies of the greatest respectability, which have been formed for the purpose of receiving and laying before the Public papers on all subjects relating to medicine. Lastly, I have been accused of allowing myself to be deceived, or wishing to deceive others, respecting the result of some of my experiments.

The number of my experiments arises from the great variety of subjects which the Inquiry embraces. It will be found on examination, that the experiments relating to any one of the subjects, requiring the employment of living animals, are fewer than it is usual to make in such investigations. It has been uniformly my aim, as every part of the preceding Inquiry demonstrates, to lessen as much as possible the suffering occasioned by it. A large majority of my experiments was made on the newly dead animal, a circumstance which my accusers have always overlooked.

The minuteness of some of my details originated in the wish just mentioned, to lessen as much as I could the suffering inseparable from such investigations, by rendering as few repetitions of the experiments as possible requisite. The chief means of doing so is to watch every step of them with strict attention, and give to the reader such details as to convince him, that if they were not frequently repeated, they were made with a degree of care which rendered frequent repetitions unnecessary. Besides, these details are often requisite to prevent errors in case the experiments should be repeated by others. Those versed in subjects of this nature know how much unnecessary suffering to animals, and trouble to the operator, their omission has sometimes occasioned. Every man of a moment's reflection must be aware, that whether such details are given or not, they necessarily have existed. We here see, I cannot help thinking, one of those instances in which, in such questions, a false and sickly sensibility is constantly attempting to usurp the place of true and rational feeling. Whom have I seen among my open defenders? men known for all the kindest feelings and the best social virtues. They look beyond the means, and perceive them directed to prevent infinitely more suffering than they occasion, and that of a higher order; for who will compare human suffering, with all its recollections, and all its forebodings, to the suffering of the animal, which feels only the present moment? These observations bring me to the consideration of a question more deserving of notice.

Many good and even dispassionate men have doubted, whether we are entitled, with a view to the welfare of our own species, to make painful experiments on the inferior animals. The earnestness with which a dispute on this subject was conducted about two years ago in the New Monthly Magazine, induced me to address to the editor some observations on it, under the signature of Philanthropos, which appeared in that Magazine in September, 1816. I shall beg leave to present these observations to the reader with little change. They relate to the general subject of cruelty to animals. I shall not omit what is said of the other instances of this cruelty, because I cannot help thinking, that in them, the gentlemen who have stepped forward in the present question, will find a more appropriate, as well as a far more extensive field for the exercise of their feelings.

I beg the reader to recollect that the following observations were published anonymously, and never meant to appear, except in the work for which they were written. They will, on these accounts, the better answer the purpose of the present address, because they must, under such circumstances, be regarded as the real sentiments of the writer. He will excuse the popular style which the pages of a magazine require.

" *On Cruelty to Animals.*

" But if our sports are destructive, our glut-
" tony is more so, and in a more inhuman
" manner."—*Guardian*, No. 61.

" Mr. Editor,

" I trouble you with the following observations, in consequence of the various papers which have appeared in the different numbers of your magazine, on the subject of cruelty to animals. I hope and trust that both parties in this dispute have expressed more than they would coolly maintain, which is very common in disputes of all kinds. I cannot believe that *Immisericors*, or any other man, who retains the faculty of judging between right and wrong, can approve of the experiment of Doctor Pitcairn, of Edinburgh, which he details. Nor can I suppose that his opponents will, on reflection, maintain, that the greatest physiologists, many of whom were equally remarkable for profound talents, and all the best feelings of our nature, are properly designated by the epithets of heartless and cold-blooded. One thing in the papers, on this side of the question, seems at first view extraordinary. They are written with the best feelings, and I have no doubt the best intentions; yet they dwell only on one species of cruelty to animals, and that the least censurable, because it is of most rare occurrence, and is perhaps the only one which admits of any justifiable defence on the plea of utility. This I think it is not very difficult to account for. What is of frequent occurrence makes little impression, so much are we creatures of habit. But to calm reflection, the frequent occurrence of the evil serves only to aggravate it. We smile at the reply of the girl who was accused of skinning the eels—that it was nothing to them, because they were accustomed to it! but her reply only indicates a greater degree of the same deception to which we are all liable, but which it is the province of reflection to guard us against. The poor girl did not reflect that it was she herself, and not the eel, that had by habit become familiar with the operation she was defending.

" As experiments on living animals are the topic of discussion in the papers above alluded to, I shall first offer my sentiments on this subject; endeavouring, on the one hand, to guard against that sympathy for the sufferers which, however amiable, tends, like all other feelings, when in excess, to blind the judgment; and, on the other hand, against that unfeeling, and, in my opinion, most unjust principle, that the greatest sufferings of the brute creation are not to be placed in competition with the slightest advantage accruing to the human race. Whatever be the feelings of individuals, or of the moment, either on the one side of the question or the other, I am convinced that there is some point between these extremes, where the general and dispassionate opinion of mankind will always be found. I believe that, among the best informed and most reflecting part of the community, the propriety of experiments on living animals under certain restrictions, will always be admitted. This opinion is founded simply on the fact, that in many instances, the suffering which they have prevented has been infinitely greater than that which they caused; instances in proof of this assertion I shall presently have occasion to adduce. I cannot regard such experiments as justifiable on any other principle. As means of satisfying curiosity, or even of extending knowledge which does not tend to prevent more suffering than they occasion, as far as I am capable of judging, they cannot be too decidedly condemned.

" Several of your correspondents have maintained that there are no circumstances, under which experiments on living animals should be performed, supporting their opinion by the maxim, that we are not to do evil, that good may come. This maxim cannot be regarded as applicable to physical evil. Nobody considers it wrong to whip a child and amputate a limb, provided they are done under proper circumstances. Those who use it as an argument on the present occasion, therefore, appear to me to beg the question; the point in dispute being,—whether experiments on living animals are, under all circumstances, morally wrong. If so, there cannot be two opinions respecting them. We shall suppose a case which has actually occurred. Men are thrown on a coast where they find nothing to maintain life but fruits with which they are unacquainted, but some of which they know to be poisonous. Is it morally wrong that they should, by experiments on their dogs, ascertain which are poisonous before they themselves eat them? Nobody, surely, can hesitate in answering this question. If so, can we censure Orfila for one of the most extensive set of experiments on living animals which has been laid before the public, made for the purpose of discovering antidotes for the poisons often accidentally taken in ordinary life, by which many human beings have been saved, and many thousands will be saved, from one of the most painful of all deaths? Or, will it be maintained that animals may be sacrificed to save one man to-day, but not to save thousands at a future period? Who can calculate what human suffering has been prevented, and how many human lives have been saved, by the experiments on living animals, for example, which taught the circulation of the blood, and thus gave to the practice of medicine, in many, particularly inflammatory, diseases, a precision formerly unknown!

" In the character of those, who have been engaged in such experiments we see a sanction for them of a different kind. Where shall we find a man of a stronger moral sense or sounder principles of religion than Boerhaave was? If such a man has existed it was Haller, whose observations on the Christian religion are esteemed even by those who devote their lives to its study, and whose letters to his daughter shew how much alive he was to all the gentler feelings; yet of all the physiologists of his time, he made most experiments on living animals, and most frequently appealed to the evidence afforded by them. Where shall we find a brighter ornament to the church or to human nature than the celebrated Hales, emphatically called the Christian Philosopher? yet he scrupled not to make experiments on living animals to improve our knowledge of physiology. Many similar examples might be adduced. Let us be cautious how we arraign the judgment of such men. Your correspondents frequently refer to the opinions of the no less celebrated Johnson. The greatest advocates of this writer have lamented the prejudices which clouded his otherwise excellent understanding, and prevented his judging with accuracy of many questions in which the feelings are concerned. I cannot help thinking that in the question before us the example of such men as those whom I have mentioned is a safer guide. While we are determining the question, whether we shall voluntarily inflict suffering on the inferior animals, we must not allow our feelings to betray us into the forgetfulness, that this question, in the present case, involves another,—shall we by such means endeavour to prevent much more extensive suffering? Let me repeat, that neither the train of reasoning which I have pursued, nor the example of the great men whom I have mentioned, can sanction experiments on living animals made from a motive of curiosity, or even for the purpose of acquiring information on subjects of subordinate consequence. By pointing out the great and good purposes which such experiments may be made to serve, they seem to afford an additional argument against having recourse to them on slight occasions. If it be necessary that animals should sometimes be sacrificed for the advancement of a science, in which the well-being of our species is so deeply concerned, it is doubly incumbent on us to protect them from wanton cruelty. This reflection naturally leads us to the other parts of the subject.

" It must be owing to that tendency of our nature, which I have had occasion to mention in the beginning of this paper, that the sense of the public seems, under certain circumstances, to admit of the lawfulness of cruelty for amusement. Are not hunting and fishing sanctioned by public opinion? Do we not see men of the best moral feeling engaged in both? It may be said in reply, that the animals destroyed are meant for the food of man, and that in hunting and fishing we only obey the dictates of nature. But why are not fish always caught by the net? Why are they exposed to the torment of the hook, in some kinds of fishing long protracted? And is there no sympathy for the animal, sometimes a fish, a worm, a frog, &c., impaled alive on the hook, and in many cases so applied as to preserve life as long as possible? Why are the sufferings of the animal prolonged in hunting, by using dogs of little greater speed than the animal hunted? Why is it sometimes caught by other means, and turned loose on purpose to be hunted? and why, when it is rare, and consequently valuable, is it saved from the dogs to be hunted again on a future day? Can any physiological experiments be more cruel than these practices, whose object is mere amusement? The train of thought into which I have been led cannot fail to make us wish that, at least, the more cruel parts of such amusements were avoided. Let me pause for a moment on the sufferings of the post-horse, frequently inflicted on very frivolous occasions, and sometimes increased even to death. The most humane will often hear unmoved the repeated lash of the post-boy's whip urging to almost impossible exertions. Such is the power of habit! To enumerate all the instances of this kind would be endless.

" There is another species of cruelty that ought not to be overlooked, which is, if possible, still more reprehensible; and which, if not actually sanctioned by public opinion, is overlooked by the public. The cruelty practised in our markets, the mere effect of carelessness, or for the purpose of obtaining the lowest of all gratifications, is disgraceful both to our age and country. Who but those daily accustomed to it, can see unmoved eels stripped of their skins, and hung up to writhe to death in hours of agony, for no other reason than that the skinning, which is otherwise necessary, saves the trouble of killing them in any other way? And who, without feelings of indignation, can see other fish cut across in various places, and left to die in similar agony, merely to improve their flavour? Many similar practices might be mentioned. These things are not, like the solitary experiment of the physiologist, of rare occurrence; they are the incidents of every day, which produces not one or two, but thousands of instances of them. There are few physiological experiments, perhaps, by which half so much suffering is occasioned, as arises from each instance of these daily, general, and I may surely be allowed to say, useless cruelties. What arguments shall we use to extenuate these and the cruelties of amusement? When shall we hear a voice, like that of Boerhaave, Haller, or Hales, raised to defend them? We must not commit their defence to any train of reasoning. They are tolerated only because we do not allow ourselves to reflect on them."

"PHILANTHROPOS."

If I may be allowed, in addition to what is said in the first part of the above paper, to speak of any thing which I have myself done, I would say, that the employment of galvanism in habitual asthma, even in its still very limited extent, and during the short period in which it has been used, has saved many times the suffering occasioned by all the experiments which led to it. Nay, I could mention single cases from which alone, if it had never been employed in any other, this might be said:—Yet let not those, who view the physiologist only in his work, imagine that he inflicts suffering on animals without reluctance. In this Treatise itself the intelligent reader will perceive sufficient proofs of that reluctance. It contains experiments only on subjects of the first importance. It contains no superfluous repetitions, and in every instance in which it could be done without influencing the result, which was the case, as I have mentioned above, in a large majority of my experiments, the sensibility of the animal was destroyed previous to their commencement. Many who did me the favour to assist me in my experiments know, that I often abstained from ascertaining points, which I wished to ascertain; because I did not consider them of sufficient importance to warrant the suffering, which the investigation would occasion. Accused as I have been, I may be allowed to state, and the truth of the assertion is easily ascertained, that there is no other instance of a physiological inquiry of equal extent with the present, in which so few living animals were employed; a fact which is alone sufficient to evince the feelings, with which it was conducted.

With respect to the objection which some make to laying the account of such experiments before the Royal Society, I admit its force; but they will do me the justice to allow, that in this I only followed the example of men of the greatest respectability and first name in our profession. Besides, my first papers having been, not only favourably received, but ordered to be published, I did not expect any opposition of this kind to other papers of a similar nature. My reason for offering my first paper to the Royal Society was, that its results contradicted those of the experiments of M. le Gallois, which had received the sanction of the Royal Academy of Sciences, and in support of which the Academy had published a memoir of above seventy pages. It will be admitted that it was natural for me, under such circumstances, to wish that my experiments should be published with the sanction of the Royal Society. I might here also urge the encouragement which has been given by this Society to such investigations, even from its earliest establishment.

The most painful part of my task remains. I do not shrink from it, because the cause of truth, no less than the duty I owe to myself, calls for it. I have been accused of allowing myself to be deceived, or wishing to deceive others, respecting the results of some of my experiments.

Of four papers which I have had the honour to present to the Royal Society, the two first and the last were ordered to be published. After the paper which was not published had been in the hands of the President for several weeks, for whose obliging conduct towards me on various occasions I must ever feel grateful, I learned, through a channel by which it was impossible for me to be deceived, not only that he had expressed an opinion of it highly gratifying to me, but that he had received a favourable report of it from those, whom he considered best versed in the subject of which it treated, to whose consideration he had submitted it.

The next intelligence which I received relating to it was, that it was not to be published by the Society; and a few days afterwards I received a letter from a friend, who is in habits of intimacy with many of the members of the Royal Society, in which he states, "I have "good reason to believe that it has been re"ported in and out of the Royal Society, that "your observations and conclusions are not "correct, and that the stomach cannot be "made to act by the process you have de"scribed." I had soon reason to believe from several other circumstances, that the President and others had changed their opinion respecting the merits of my paper.

At this time I viewed these circumstances with surprise. They appeared to me inexplicable, for I could not conceive the possibility of what the following incident disclosed. One thing alone I was assured of, that such men as those who had, in the first instance, given an opinion respecting my paper, would not change it on slight grounds.

As the paper had been written from detached notes, and I had kept no regular copy of it, I wrote to one of the Vice-presidents of the Society, requesting, that as it was not to be published, it might be returned, or, if that could not be done, that a copy of it might be sent to me. The Vice-president's letter is now before me, in which he says,—" I applied to the " council for a copy of your paper, and gave " directions to the clerk to copy it." In addition to the copy of my paper, thus made out by order of the council, which also now lies before me, I found, to my great astonishment, written in the same hand with the copy of the paper, the following observation of the clerk with the subjoined account of an experiment.

" *N. B.* The following is a copy of a paper, " written in a different hand, and pinned in " page 13, over part of the account of Exp. 3 *, " without any reference to the text what- " ever."

" Two rabbits, which had had no food for " seventeen hours, were allowed to eat parsley. " The nerves of the   were then divided " in the neck of each. One of them was allowed " to remain quiet. A slip of tin foil was con-

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* The first of my galvanic experiments.

" nected to the lower divided ends of the nerves
" of the other rabbit, and another piece of tin
" foil, an inch square, was applied to the abdominal muscles over the stomach, and under the integuments, by means of a wound
" in the latter. The tin foil over the stomach
" was connected with a wire communicating
" with one end of a voltaic battery of twenty
" plates, and occasional contacts were made
" (about three or four times a minute) between
" a wire connected with the other end of the
" battery and the tin foil in the neck. The influence of the battery was sufficiently strong
" to excite slight contractions of the muscles
" of the fore legs. This process was continued
" during five hours, at the end of which period
" both rabbits were killed.

" On examining the stomach of the animal,
" which had been subjected to the influence of
" the battery, it was found much distended
" with food, the parsley was principally in the
" cardiac portion, and near the œsophagus it
" appeared to have undergone no alteration;
" and below this it was mixed with the other
" food in the stomach, so that no accurate observation could be made on it.

" The stomach of the other rabbit was examined by the side of the first, so that they
" might be compared together, and the appearances were precisely the same with those
" which have been just described. The con-

" traction in the centre of the stomach was
" somewhat greater in the galvanised stomach
" than in the other."

The above addition to the copy of my paper, sufficiently explained the circumstances attending its presentation. An account of an experiment, which, on a superficial view, or to those whose attention had not been particularly directed to the subject, appeared to be a repetition of my galvanic experiments, affording a result in direct opposition to them, was laid up in the archives of the Royal Society; and, without any thing having been said to me on the subject, pinned to the account of my experiments; from which it would appear that I had either been deceived in their result, or wished to deceive others. I had either been in the most unaccountable degree careless or uncandid.

How far the experiment just laid before the reader warrants this conclusion, a very short comparison of it with my galvanic experiments will shew. The inference afforded by them, that when the eighth pair of nerves is divided, and the digestive power of the stomach thus wholly suspended*, it may be renewed and supported by passing a continued stream of galvanism through the stomach †. Let us inquire whether the experiment in question interferes with this inference. In repeating my experiment, the first step of course is to divide the eighth pair of nerves. Without this the animal is not in the state which affords an opportunity of making the experiment at all. If the nervous influence which nature supplies is not withdrawn from the stomach, no addition made to it can be of any avail in promoting the process of digestion. This organ already possessing as much nervous influence as the fluids supplied to it require, can make use of no more. The first question then with respect to the above experiment is,—Were the nerves of the eighth pair divided previously to the application of the galvanism? *They were not.* The author does not pretend to say that they were. He leaves a blank for the name of the nerves divided. He mentions none of the symptoms which uniformly follow the division of the eighth pair of nerves, and the state of the contents of the stomach, which he describes, is such as it is never found to be, after the animal has survived the division of these nerves for five hours *. The remaining question is,—Was a continued stream of galvanism sent

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* The muscular power of the stomach remains after the division of these nerves. See page 154.

† My experiments also relate to the lungs, but the author

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of the above experiment confines his attention to the stomach.

* See pages 154 and 155 of this Treatise. The contents of the stomachs of both rabbits were in a perfectly healthy state. The author of the experiment was deceived in supposing any

through the stomach*? *It was not*, for the author of the experiment states that only "occasional contacts were made (about three or "four times a minute) between a wire connected with the other end of the battery and "the tin-foil in the neck." *In what essential respect then does this experiment resemble my galvanic experiments*†?

My first impulse on the receipt of the copy of my paper, was to address a letter publicly to the President of the Royal Society, requesting an explanation of the above most extraordinary occurrence. On more mature reflection, however, and by the advice of a distinguished Fellow of the Royal Society, to whom I had mentioned all the circumstances, and my intention of publicly addressing the President, I resolved to adopt a less painful, though more tedious means of securing to my experiments the credit to which I knew them to be entitled. I therefore merely published above a year afterwards, in an Appendix to this Inquiry, the account of the experiment which I had re-

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part of the old and new food mixed together by a circumstance explained above. See pages 142 and 143.

[*] I cannot help remarking, that the introduction of the tin-foil under the skin, perhaps the most painful part of the experiment, is an instance of useless cruelty, the skin being a sufficiently good conductor of galvanism.

[†] I have not thought it necessary to mention several deviations from my experiments of less importance, none of which, however, were allowable.

ceived, without saying from what quarter I had received it, pointing out that it did not bear on the subject in question. I was convinced that its author, of whom I am to this moment ignorant, would see or hear of this notice of it; and hoped, that if he could not bring himself openly to acknowledge the irrelevancy of his experiment, he would at least take some step to do away the impression it had made. It is now, however, nearly a year since the first edition of my Inquiry appeared; and I have means of knowing that no such step has been taken. I therefore recur to my original intention, and thus publicly address the President and Council of the Royal Society; satisfied from the proofs of their candour which I have in other instances received, that they will do what the cause of truth requires from them,—That they will call on the author of the above experiment, either to shew in what respect it opposes the result of my experiments, or publicly to confess his error.

THE END.

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